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THE STUDY OF STATISTICS IN COLLEGES AND TECHNICAL SCHOOLS.

BY FRANCIS A. WALKER, PRESIDENT.

DURING the past twenty or even ten years there has been an astonishingly rapid development of historical and economic studies in our higher institutions of learning. At the recent meeting of the American Historical Association in Washington, President Adams of Cornell presented an account of the work at present done in history in our leading colleges. To one who graduated thirty years ago, this account reads strangely enough. In place of, at the most, a single brief series of lectures on the so-called philosophy of history, we now find course after course of advanced historical study, with the free use of the library and with the most improved methods of the German Seminar, offered to students as a leading feature of their undergraduate work. Several of our American colleges have developed these courses in such variety, to such an extent, and with such a wealth of material, that they might not unfairly be called schools of history; and every year sees this carried further and further, with continually better and better results.

In the kindred department of economics, the progress made in recent times has been second only to that which we have noted regarding history. Indeed, the progress in the case of economics has been smaller only in respect to the number of colleges which have undertaken extended courses. In the institutions where both the new departments of study and research have been given scope, the extent to which political economy has shared in the growth of the past few years can scarcely be said to be inferior to that obtained by history. Neither in the

ability and reputation of the instructing staff, nor in the number and variety of courses offered, nor in the attendance and enthusiasm of the classes formed, does political economy yield to history at Harvard, or Yale, or Columbia, or Cornell. Together these two closely related departments make up a very large and constantly increasing part of the modern university.

Unfortunately, while this rapid development of historical and economic work has been going on, a branch of study which has the highest virtue at once to train the hand of the historical or the economic scholar and to furnish him with professional tools of the first importance has been almost wholly neglected. I refer to statistics, whose very methods are hardly known to the great majority of our economists and historians; and which is still to have its first chair founded in an American college. There are, indeed, a few schools where a little elementary instruction has, of recent years, been given in the use of figures as a means of testing sociological conclusions; but in no one of them has a full, proper course of statistics been established. It cannot be long, however, before the growing interest in economics and history will compel the recognition of statistics as a distinct and an important part of the curriculum of every progressive institution. The main difficulty will be to find the men who have had the training, at once severe and liberal, which will qualify them to inspire and direct these studies.

The three uses of statistical study, aside from its value as a means of discipline, are, in their order from lowest to highest, as follows: —

First, to enable the student to detect the fallacies in conclusions drawn by others from quantitative statements concerning human affairs, actions, interests, in which adventitious elements lie concealed, or from which something essential, or at least relevant, has, by inadvertence or dishonest design, been excluded.

Secondly, to enable the writer or the speaker upon politics, economics, history, or sociology safely and effectively to illustrate and emphasize his conclusions, drawn from a study, itself perhaps mainly or wholly non-statistical, of the subject to which he devotes himself.

Thirdly, statistics may, under proper direction and with due safeguards, be used for the discovery of social laws.

The first of these objects could perhaps only be fully attained through those long and weary stages of training which would be required to qualify one for the highest exercise of the statistical faculty, as last stated; but a very large part, at least, of the result desired, can be reached by a little very elementary instruction. To take an illustration from another department of study, we may suppose that an adequate

course in logic, sufficient to make a man, otherwise well trained, a sound and accomplished reasoner, might be compassed in a certain number of exercises per week, continued through two academic years. Yet, if time be not afforded for such a course, a great deal might be done to enable the student to detect false conclusions on the part of others, and to save him from the grosser errors of reasoning in his own writing or speaking, by means of a dozen hours devoted to fallacies. In much the same way, if a full course in statistics cannot be given, a few exercises upon the abuses of statistics may at least serve to keep one for life from a certain class of blunders from which men of the greatest acuteness and learning might not otherwise be exempt.

Let us take an illustration of the sort of errors against which the merest elementary study of statistics might prove a sufficient protection. A meritorious writer adduces as a proof of the great fall of prices which took place in New England between 1630 and 1640, that a cow which, at the former date, was worth £25 to £30, would, at the latter date, have brought but £5 to £6. Now the bare facts here are not in dispute; nor is it to be questioned that a fall, a great fall, in prices did take place in New England during the period referred to. Yet the statement quoted contains a gigantic blunder, — a blunder which a student of statistics would probably be incapable of making. In 1630, the value of a cow in New England represented the immense cost and risk of bringing an animal, by a slow-sailing vessel, thousands of miles, through comparatively strange seas, into a foreign climate. Ten years later, the value of a cow represented only the cost and risk of rearing her upon the soil. The cow of 1630 might still be living, surrounded by ten, twenty, or fifty of her descendants, born in New England.

Errors of this type are countless. They occur in the writings, they are heard in the speeches, of men learned and otherwise acute, but who have never been trained to detect the fallacies that lurk so cunningly under all groups of figures. Volumes might be filled with instances of statistical blunders of a class which a very elementary course would forever render impossible to any careful writer or speaker. Such a course would embrace a host of illustrations, affording examples of the kinds of error which especially beset the use of figures for sociological purposes, and would direct the attention of the student to the best means of exercising care and pains in escaping them.

It is easy to say that, if statistics be in truth such "kittle cattle," if danger lurk thus under every group of figures relating to social and economic matters, it would be better to eschew statistics entirely. But mankind will not consent to give up an agent of such power, because of

the abuses to which it is subject. If all men at once honest and candid were to forbear to employ statistics in such discussions, lest peradventure they should lead some astray, we may be sure that all the dishonest and uncandid would resort to their tables and diagrams with redoubled zeal. There are few instincts more strong than that which urges men to give a quantitative expression to the results of human experience. Men will do it, or have it done for them by others. No warning as to the possible errors of such evidence can prevent this appeal, or diminish the eagerness with which it will be made. What we must needs do, if we will promote the truth, is to instruct and exercise the citizen, as far as we may, in the scrutinizing, sifting, and testing of alleged statistical proofs.

What has just now been said brings me to the second of the objects enumerated as to be sought through the study of statistics: namely, to qualify and prepare the future writer or speaker upon political and economic and social questions safely and effectively to illustrate his conclusions, derived perhaps through processes mainly or wholly non-statistical. I have said that the instinct which leads men to seek quantitative statements for the results of human experience is one of the strongest in our nature; and that people will have this done, whether it is to be done rightly or wrongly. He then, who, in addition to the merits of sound and just thinking on social subjects, possesses the power of aptly using statistics, acquires thereby a great advantage, whether in exposition or in controversy, over almost any one, however gifted or brilliant in argument or in the graces of speech, who has not this peculiar faculty. All who have widely observed audiences gathered for the purposes of political discussion must have seen the almost ludicrous liking which they have for statistical statements. The crudest thinking is oftentimes carried through by an array of worthless statistics, which would not bear a moment of cool, critical investigation. Quantitative statements that are scarcely even relevant to the subject are, for popular purposes, better than none. What power, then, can a real master of statistics wield over his hearers! Attend a meeting where Mr. David Wells is speaking, and see how he holds the crowded audience in close attention for two hours, with no help from rhetoric, elocution, or gesticulation, merely by the strong, vivid, effective way in which he marshals figures. There are few orators who can so completely command the thoughts of their hearers, for the same length of time, by all the graces of speech, or even by stately and beautiful thoughts, as this publicist, whose style of speaking is not merely unfinished, but positively bad. Mr. Cobden owed very much of his extraordinary power to the

same cause. Mr. Gladstone is an even more remarkable example of the virtue of this art. Unthinking people say that he must be a wonderful orator, because, in spite of the serried masses of figures which belong to a budget speech, he has more than once held the House of Commons strictly silent and attentive for the space of three or four hours. The fact is, that that remarkable success was not obtained in spite of statistics, but by reason of them. There is no spectacle on which men, whether more or less educated, look with more breathless interest than the marshalling of a vast array of figures, which move and take their allotted place, in natural succession and in due order, at the bidding of a real master of that art.

Since so much popular interest attaches to the use of statistics in addressing any large audience, whether from the platform or from the author's or editor's desk, it is clearly worth while for every person who is under training to become a writer or a public speaker to undertake all the instruction and practice which may be necessary to enable him to put together the facts and figures which relate to any chosen subject at least clearly and correctly. We cannot all be Cobdens, Gladstones, or Wellses; but every educated man can learn to construct tables and diagrams which will bear the test of a fair scrutiny and liberal criticism. To do aught in the way of statistics at which fools will not peck, is of course beyond any man's power.

Those who have never tried their hand at statistical work will fail to appreciate the difficulties to be encountered at the start, and the frequently recurring need of going back and beginning all over again. To go to a series of extended tables with multitudinous subdivisions, in which a given total is distributed among many classes, and to take therefrom just what you want, no more, no less, and no other, — to make sure that your parts when put together will form a whole, and that no direction conveyed by the heading of a single column has been neglected, — is a task for which men must be trained, and in which they must be practised, going from simple and easy examples to complex and difficult ones, by patient steps. The great majority of editors and writers for the press, the great majority of legislators and public speakers, either fail on such work, or, as is most likely, judiciously avoid the attempt, even though statistical matter altogether relevant to the subject, and which might be made most interesting to their readers or hearers, lies on every side of them. In my long experience in office at Washington, nothing struck me more forcibly than the helplessness of Congressmen — even, with few exceptions, the acutest and best trained — to get up the figures for their own speeches. No matter

how clear their conception of the positions they wished to present, few of them could readily and confidently resort to the government publications at hand for the statistical materials with which to illustrate and enforce their views; and the gratitude with which they would accept and acknowledge some trifling assistance from a well trained clerk was almost ludicrous. I do not intend any disparagement by this statement. Statistics have a language of their own, and he who would use them must first learn that language; and this is as yet taught scarcely anywhere.

A striking example of the liability to mistakes which constantly besets the compilation of statistical tables was afforded in a book published, some years ago, under the title, "The Statistics of the United States." The plan of the work was a good one; such a book was needed; but the author evidently had not had the training requisite safely to carry out his scheme without falling into the gravest errors. For instance, the work undertook to present the expenditures of the United States for each year since the formation of the government. The figures used were taken directly from the finance reports of the Treasury Department, and were hence of the highest official authority. Unfortunately, however, the compiler went for this purpose to the column of "Gross Expenditures," and transferred the figures he found there into his table. The result was that for some years he was out of the way by several hundreds of millions of dollars, since during these years the Treasury issued large loans to pay off other loans contracted during the war at high rates of interest. Thus, for 1868 this writer gave the expenditures of the government as $1,093,079,655, — a very expensive government indeed for a time of profound peace! The facts were as follows. The "net ordinary expenditures" of the government that year were $202,947,734; there was paid from the Treasury, in bond premiums, $10,813,349; and, as interest on the national debt, $143,781,592; making the total expenditures of the government on these accounts, $357,542,675. In addition, the Treasury redeemed bonds to the amount of $735,536,980; and this, mainly, out of the proceeds of fresh loans, at lower rates of interest. All this vast sum, more than twice the actual expenditures of the government, even after including bond premiums and the current interest on the public debt, was embraced in the financial statement of the last year of Mr. Johnson's administration. This mistake was committed in connection with each successive administration, from Washington's down.

It is needless to say, that blunders of such a magnitude completely destroyed the prestige of the book; and that, although it was intended

to be issued from time to time, with the facts and figures brought down to date, it was never heard of again.

Another example of statistics rendered actually delusive by the neglect of elementary considerations is found in a recent work on State and Municipal Taxation, a book which, in many of its views and suggestions, makes a valuable contribution to economic literature, but is, statistically, very faulty. Thus, in a "comparative table," showing the "principal receipts, total receipts, and total expenditures" of certain leading cities, New York is put down for $73,309,884 of total receipts, in 1886, and for $71,750,743 of total expenditures. Now the fact is, that nearly twenty millions alike of the receipts and of the expenditures represent nothing but temporary loans, contracted and paid during the year. City taxes come in mainly during a brief period. In order to prevent the necessity of keeping a vast sum of money in the treasury for months together, the government properly borrows in the "dry season," and liquidates its obligations when the taxes set in like a flood. Yet this fact was, in the work referred to, allowed to swell the expenditures of the city more than one third. Had the city treasurer found it expedient to borrow ten millions more for one, two, or three months, this would have carried the "expenditures" of New York up to eighty-one millions!

Instruction directly intended to qualify a student to use statistics, and to compile tables with ease, confidence, and accuracy, is now given at Harvard University, Columbia College, the Institute of Technology, and probably elsewhere. The pupil is taught to look up the data relating to a given subject, as these may be found scattered through long series of official reports; to bring the various statements together; to examine them as to their proper comparability; to test their accuracy by all means which may be available; and to put them together into tables. The student is further taught to work out the percentages involved, and to set one class of facts into relation with others; as, for example, to compute the ratio of valuation, or of expenditure, or of mortality, to each million or each thousand of the population concerned; and, finally, to make diagrams or charts, which shall exhibit graphically the several elements, taken in their due proportions, as ascertained by the investigation. In none of the higher institutions, however, is this branch of study carried as far as it ought to be; nor are all the methods of instruction in this department yet worked out to their greatest efficiency. Still, the good work has been well begun; and the constantly growing appreciation of the ability to compile and to use statistics for the purposes of political, economic, and social discussion cannot fail to cause a rapid

development of this feature of the college course. The American Statistical Association, under the able secretaryship of Prof. Davis R. Dewey, is doing much to promote this study; and it is the desire of its officers that its Journal may become to a considerable extent at once the organ of communication, of suggestion, and of friendly criticism among the working statisticians of the country, and the repository of the best essays in this line from our leading colleges and universities: affording, in the latter way, a great impulse to the study of statistics "where it will do most good," namely, in connection with the academic pursuit of history and economics.

The scope of this paper does not include a discussion of the subjects and the order of studies designed to give the investigator the power to discover statistically the laws which govern the action of social and economic forces. Such a course would necessarily be long and severe. It should, for the best results, embrace the highest mathematics of our American colleges, and should be largely directed to the development of the biological sense. The number of those who, otherwise than as a means of mental training, would have occasion to undertake such a course, would necessarily be small. There is reason to wish that all citizens, from the highest to the lowest, might undergo so much of training in statistics as should enable them to detect the errors lurking in quantitative statements regarding social and economic matters which may, by tongue or by pen, be addressed to them as voters or as critics of public policies. Comparatively few of these, however, will ever have occasion to prepare such statements for themselves, and would thus have use for the special faculty which is required for the compilation of statistical tables and diagrams. Far smaller still will be the number of those whose natural endowments and whose chosen pursuits would justify the long and laborious training, the patient practice, and the acquisition of the large and various learning, which alone can qualify the student of history, of sociology, or of political economy confidently and surely to educe from thousands of pages closely packed with figures some hitherto unsuspected law of human life or conduct.

EXPERIMENTS ON THE PREPARATION OF BOILED LINSEED OIL.

BY FRANK H. THORP, S. B.

THE composition and properties of the drying oils have been but little studied. About twenty-five years ago, Mulder[1] took up this subject and published his observations. Within the last few years other chemists have investigated the subject, but the particular changes which take place in the so called "boiling" of linseed oil have received little attention.

The drying of this oil is due to its oxidation, and the oxidation is greatly hastened by the "boiling." The dryer, or substance added to the raw oil, partially oxidizes the oil during the boiling, and when a thin layer of the boiled oil is exposed to the air it rapidly hardens owing to the further oxidation of the oil. The dryer dissolved in the oil acts, perhaps, as a carrier of oxygen from the air to the oil.

The dryers most commonly used are litharge, pyrolusite, borate of manganese, and zinc white. Recently the oxalate of manganese has been proposed for this purpose.[2]

According to Mulder, some of the linoleic acid is converted to linoleic anhydride during the boiling. This anhydride is a tough, elastic body. At the same time, some of the metal of the dryer forms a salt with the remainder of the acid, and this salt gives hardness to the varnish.

Hazura and Bauer[3] find that the rate of oxidation, and consequent hardening of the oil, depends on the ratio of linoleic and linolenic acids present.

When linseed oil is oxidized, it undergoes very marked changes in its physical properties; the color becomes darker, the oil becomes more viscid, and there is an increase in weight, sometimes equalling eight per cent. Some carbon and hydrogen are also driven off. These changes occur also during the process of "boiling." According to

[1] Die Chemie der austrocknenden Oele, Berlin, 1867.
[2] Casteloz, Bull. de la Soc. Chimique, Vol. L. pp. 597, 645.
[3] Monatsh., Vol. IX. p. 459.

Cloetz[1] the carbon and hydrogen eliminated do not all pass off as carbonic acid and water, but partly as an irritating vapor resembling acrolein. He has shown that the glyceryl is destroyed.

A good varnish must be thin enough to apply with the brush, must dry without cracking and with rapidity, and give a clear, colorless film, which should be somewhat flexible. Oil varnishes give tougher films than volatile varnishes.

Preparation of Boiled Oils.

The oil used was a very light yellow cold-pressed, raw, Calcutta linseed oil. The same quantity of oil, 50 c.c., was used in each experiment unless otherwise noted. The weight of 50 c.c. of this oil was found to be 45.7 grams. The quantity of dryer used in each case was noted.

The general process of boiling in each case was as follows.

The measured oil was heated in glass beakers of about 150 c.c. capacity, which were set in a pan of fine sand as closely as they would stand without touching. The sand bath was heated by gas burners beneath. Thus from four to six beakers were heated at one time. By the use of a sand bath, the temperature of the oil could be regulated better than by direct heating, and the danger of the taking fire of the oil was reduced. By raising the sand around the beakers to about one half the depth of the oil, the whole mass could be very evenly heated. The oil was frequently stirred during the heating. In the course of the experiments it was found that the size of the beaker and the quantity of oil used had a marked influence on the temperature. With a beaker of 250 c.c. capacity and containing 100 c.c. of oil, the temperature was found to be about 30° higher than in a beaker of 150 c.c. capacity containing 50 c.c. of oil, under similar conditions. The temperature in the larger beaker was also much more even.

As the presence of small quantities of water in the dryers caused violent ebullition and frothing when they were added to the hot oil, they were carefully dried before being stirred into the hot oil. In those cases where the dryer was added to the cold oil and slowly heated with the oil, it was seldom dried, as all water escaped before the oil was hot enough to froth or spatter. But salts with "water of crystallization" always caused some disturbance, since this water does not escape until the temperature is quite high. With clear raw oil there is no ebullition until a very high temperature is reached. At 220° small bubbles

[1] Bull. de la Soc. Chimique, Vol. III. p. 41.

begin to show around the edge of the oil surface, and white irritating vapors of very disagreeable odor begin to come off. But active boiling of the oil does not take place under 360°, which was as hot as these oils were in any case heated. In general, the best results were obtained with temperatures between 230° and 275°.

Lead Salts.

Litharge. — Six experiments were made with quantities of litharge, varying from one half of one per cent to two per cent of the weight of the oil, and temperatures varying from 220° to 300°. Two of these experiments yielded fairly good boiled oils. The first was made with one per cent of litharge and at a temperature of 250° for two and one quarter hours; the second, with about two per cent of litharge and a temperature of 220° for one and a half hours. The remaining four experiments did not give good boiled oils.

Lead Peroxide. — One experiment only was made, at a temperature of 220°, but the resulting oil was worthless on account of its exceedingly dark color.

Red Lead. — Two experiments, at temperatures from 220° to 285°, gave unfavorable results.

Lead Carbonate. — Three experiments, with about one per cent of carbonate in each, and temperatures from 225° to 250°, were made. One, in which the temperature was kept at 225° for two hours, gave a fairly good boiled oil; the others were worthless.

Lead Nitrate. — One experiment, with two and one half per cent of nitrate and a temperature of 250°, resulted in a worthless mass of tar.

Lead Sulphate. — One experiment, with about two and one half per cent of sulphate and a temperature raised to 300° for nearly two hours, produced a very thin turbid oil, of no value. The sulphate did not decompose, even at this high temperature.

Lead Chloride. — Three experiments were made with the chloride, the quantities used varying from one to two and one quarter per cent, and the temperature from 250° to 360°. The first, with two and one quarter per cent of chloride and a very high temperature for one and three quarters hours, yielded a very sticky and elastic gum-like mass. In the other two cases, with lower temperatures, there appeared to be but little action on the oil.

Lead Oxalate. — One experiment, with about two and one quarter per cent of oxalate and a temperature at one time over 300°, yielded a thin and turbid oil of no value. Apparently the salt did not decompose. The oil did not dry.

Lead Acetate. — One experiment, with three per cent of acetate and a temperature of 300°, gave a very dark-colored oil which dried very slowly.

Lead Borate. — Two experiments, with about two per cent of borate and temperatures of 220° and 300° respectively, were made. The first gave a dark brown oil, and the other a dark reddish-brown oil. Both oils were quite fluid.

Lead Tartrate. — One experiment, with about two and one half per cent of the salt and a temperature of 270°, was made. The tartrate did not decompose until a high temperature was reached, and the boiled oil was very dark-colored and viscid.

Mixtures. — Two experiments with mixtures of litharge and red lead were made. Two per cent of each oxide was used, and the temperatures kept below 250° in one case, and 230° in the other. The boiled oils were quite fluid, but very dark-colored.

Two experiments with mixtures of litharge and acetate were made, using two per cent of each and keeping the temperatures as in the above cases. The results were better with these, although the oils were quite deeply colored.

Zinc Salts.

Zinc Oxide. — Two experiments, with one and two per cent of oxide respectively, and the temperature at 250°, were made; both gave turbid oils which did not clarify satisfactorily. The colors were light brown. These oils dried very slowly.

Zinc Sulphate. — Two experiments were made with this salt, using nearly four per cent in one case and three per cent in the other. In the latter case the sulphate was added to the cold oil and heated with it. The temperature in this case was raised to 285°. The oil was poor. In the former case the temperature was kept below 230°. The oil was light red in color, and the sulphate appeared to have very little action.

Zinc Carbonate. — One experiment, with three per cent of the salt and a temperature of not over 230°, gave a poor result. It was, in fact, much like the oxide.

Zinc Acetate. — Two experiments, with two and four per cent of acetate and temperatures between 235° and 280°, gave red oils, which were clear and fluid, but slow-drying.

Zinc Chloride. — One experiment with this salt resulted very similarly to the first lead chloride experiment. A semi-solid mass was formed.

Zinc Borate. — Three experiments, with two per cent of the salt and a temperature not over 240°, gave fluid dark red oils, which dried slowly.

Zinc Oxalate. — One experiment showed that the oxalate did not act on the oil after prolonged heating at 260°; on raising the temperature to 300°, the oil was converted into a mass of tar.

Zinc Citrate. — One experiment, with three per cent of the salt and the temperature at 230°, gave a fairly good, clear oil, which dried rather more rapidly than the oils prepared with the other zinc salts.

Zinc Tartrate. — One experiment, with three per cent of the salt and a temperature of 300° was made, but the salt appeared to have very little action upon the oil.

Manganese Salts.

Manganese Binoxide. — One experiment, with one per cent of the oxide and a temperature not over 220°, gave a very dark-colored boiled oil.

Manganese Carbonate. — One experiment with this salt, at a temperature not over 250°, gave a reddish-brown boiled oil, much like the best oils obtained with litharge.

Manganese Acetate. — Three experiments, with quantities of the salt from one to two per cent and temperatures from 225° to 250°, gave red boiled oils of varying depth of color. All dried quickly, and were good boiled oils.

Manganese Borate. — Two experiments, with one and three per cent respectively of this salt and temperatures of 230° and 220°, gave the best oils obtained in the entire investigation. The boiled oils were clear, fluid, and light colored. They dried quickly, and gave good films.

Manganese Sulphate. — Three experiments, with from two to about three per cent of this salt and temperatures from 240° to 300°, gave varying results. At 300° the sulphate decomposed rapidly, and tarred the oil; at the low temperatures there was less action, and good clear oils were obtained.

Manganese Oxalate. — Five experiments, with from one to three per cent of the salt and temperatures from 230° to 315°, gave rather unsatisfactory results. At the lower temperatures the oxalate did not appear to decompose, and the oils were turbid from sediment, which did not settle on long standing. These oils were light in color, but dried rather slowly. At the high temperatures, there may have been some decomposition of the salt, but the oils obtained were also turbid, and, in one case, quite dark-colored.

Manganese Citrate. — One experiment, with three per cent of the salt and the temperature at 230°, gave a very dark-colored oil.

Manganese Tartrate. — Two experiments, with from two to three per

cent of tartrate and temperatures from 230° to 260°, were made. The first gave a very good oil; the other did not.

Manganese Formate. — Two experiments were made with this salt. In the first, two per cent of the salt was used, and the oil was converted into tar. In the second, one per cent of salt was used and the temperature kept below 200°, the time of heating being about one hour. The boiled oil obtained dried very fast, but was very dark brown in color. The result was not good.

DRYING TESTS.

Dryer.	Quantity of Dryer used.	Time of Boiling.	Time required for Drying.	Character of the Film.
	Grams.	Hours.	Hours.	
Litharge	1.0	2¼	6	Almost colorless.
"	0.2	2¼	10	" "
"	0.8	1½	10	
Lead peroxide	1.072	1½	Several days.	Deeply colored.
Lead chloride	1.247	2½	24	Somewhat colored.
Red lead	1.024	2½	24	Deeply colored.
Lead oxalate	1.323	2¼	Did not dry.	
" tartrate	1.6	2¼	24	Deeply colored.
" acetate	1.46	2¼	12	Somewhat colored.
" borate	1.105	1½	20	Slightly colored.
" carbonate	1.197	2	10	" "
Zinc oxide	0.5	2¼	45	Nearly colorless.
" sulphate	1.987	2½	45	" "
" sulphate	1.5	2½	45	Yellow in color.
" acetate	1.0	2¼	40	Colorless.
" borate	1.0	2	40	Nearly colorless.
" "	0.5	1½	46	" "
" "	0.5	1½	46	" "
" citrate	1.5	2½	36	" "
Manganese acetate . .	1.0	2¼	20	Nearly colorless.
" borate . . .	1.625	2¼	20	Colorless and hard.
" sulphate . .	1 72	2	40	Colorless.
" oxalate . .	1.64	2	40	"
" acetate . .	0.5	2	20	Deeply colored.
" borate . . .	0.5	1	20	Colorless.
" acetate . .	0.5	1¼	20	"
" oxalate . .	1.5	2½	36	"
" sulphate . .	1.5	2½	36	"
" oxalate . .	1.0	2¾	48	Yellow in color.
" citrate . . .	1.5	1½	24	Dark colored.
" tartrate . .	1.0	2½	24	Colorless.
" formate . .	1.0	1	24	Somewhat colored.

Small plates of glass were used for these tests. These were coated with a film of the oil, and were then placed in a light room to dry. The temperature of this room varied somewhat, but was commonly about 70° F. during the day. Generally, from a dozen to twenty samples were tested at a time. The films were considered to be dry when they could be handled without leaving finger-marks upon them. The plates were examined at intervals of several hours, and only relative results as to the time of drying were expected. The chief points looked for were the color and hardness of the film. The results are given in brief in the foregoing table.

Examination of the Boiled Oils.

The analyses given below were made in the hope of throwing some light upon the relation between the quantity of dryer dissolved and the rate of hardening of the oil.

For the determination of the lead the following method was used. A weighed amount of oil was carbonized in a small evaporating dish, and then ignited over a Bunsen lamp until all the carbon was burned off. The residue was digested with hot dilute nitric acid, and filtered. The lead was then precipitated as sulphate, with the usual precautions, and weighed. The following results were obtained.

Boiled Oil.	Quantity of Dryer added.	Per cent of Metallic Lead in the Boiled Oil.		
		Calculated.[1]	Found.	
	Grams.		I.	II.
Lead carbonate	1.197	2.03	1.392	1.477
Lead acetate	1.466	2.23	1.396	1.338
Litharge	0.2	0.406	0.244	—
Lead borate	1.105	2.03	0.982	0.998

From the drying tests it will be seen that the first two oils in the above table dried in nearly the same time, and gave very similar results.

[1] The quantities in this column are the calculated per cents of metallic lead which the boiled oil would have contained if all the salt added to the raw oil had been dissolved in the boiled oil. But in no cases was the dryer entirely dissolved in the oil, a sediment of undecomposed salt of greater or less quantity always settling on the bottom of the bottles upon standing several days. Only the clear oil from the upper part of the bottle was used for these analyses.

But the third oil dried in half the time required for the fourth, although the latter contains more lead.

In the determinations of manganese in the oils, the following method was used. The oil was weighed and charred in an evaporator. It was then ignited over a Bunsen lamp until all the carbon was burned off. The residue was digested with dilute hydrochloric acid and heated nearly to boiling, filtered, and neutralized with ammonia. Some ammonium chloride was then added, and then an excess of sodium phosphate. The precipitate was filtered off, dissolved in hydrochloric acid, avoiding an excess of acid, and the solution heated to boiling. Ammonia was then slowly added to the boiling solution until a large excess was present. The precipitate was boiled, without filtering, for ten minutes, to convert the phosphate of manganese to a scaly form, and was kept hot for about one hour longer. It was then filtered off, washed with dilute ammonia, ignited to the pyrophosphate, and weighed. The following results were obtained.

Boiled Oil.	Quantity of Dryer added.	Per cent of Metallic Manganese in the Boiled Oils.		
		Calculated.[1]	Found.	
	Grams.		I.	II.
Manganese sulphate	1.72	1.37	.045	.038
Manganese borate	1.62	2.06	.208	.190
Manganese acetate	0.5	0.347	.248	.256
Manganese oxalate	1.5	1.034	—	.030
Manganese tartrate	1.5	0.670	.049	.043

From the drying tests it will be seen that the first and fourth oils above analyzed dried very slowly, while the last one, with about the same quantity of manganese, dried in about one half the time required for the first and fourth.

Conclusions.

Lead dryers always give the oil a deep color, which shows more or less in the film.

Zinc dryers do not appear to act on the oil to any great degree, as oils thus prepared dry slowly and do not give very hard films.

[1] See note to the preceding table.

Manganese dryers give the best results in all respects. Litharge gives the best results of the lead dryers, the oil being quick-drying and the film hard, and, if not overheated, the oil is but moderately colored. Of the zinc salts, the acetate seemed to give the best result, although the borate and citrate were nearly as good. The borate and acetate of manganese gave the best results obtained. The acetate requires careful use, for, if heated much above 230°, it gives a deep color to the oil, owing apparently to the formation of tar. The borate undoubtedly gives the best boiled oil for all purposes.

The oxalate is difficult to decompose, or at least has little or no action on the oil until a very high temperature is reached. In two experiments the quantities of borate and oxalate used and the temperatures of each were nearly the same, but the borate gave a good oil, while the oxalate did not.

The chlorides, nitrates, and sulphates do not make good dryers. The first two have too violent an action on the oil, while the last are very difficult to decompose, requiring a high temperature.

There appears to be no advantage in the use of formates, citrates, or tartrates. The first two are apt to produce much tar, and the last are difficult to decompose.

No very definite conclusions can be drawn as to the relation between the quantity of dryer dissolved and the rate of drying of the oil. From the few analyses made there would appear to be some relation here; but in two cases, lead borate and manganese tartrate, exceptions were found. The lead borate gave an oil drying much more slowly than was to be expected, while the manganese tartrate oil dried with comparative rapidity. The quantity of manganese dissolved appears to be much less than the quantity of lead taken from lead dryers. Two tenths of one per cent of manganese appears to give a good drying oil, while about one per cent of lead occurs in the best drying oils.

DATA AND PLOTS OF VARIOUS INCANDESCENT LAMPS, TOGETHER WITH AN IMPROVED METHOD OF TESTING.[1]

BY WM. L. PUFFER, INSTRUCTOR IN PHYSICS.

SINCE the appearance of the report of the Franklin Institute on incandescent lamps, in 1885, there have been no published measurements either of the efficiency or of the distribution of light of the best known lamps now in the market. But the market lamps of to-day are much better than those of 1885, so much so, in fact, that the Franklin Institute data, although very valuable as showing the state of the art in 1885, are of no especial value for use with the lamps now manufactured.

The following data and curves have been calculated and plotted from the observations made by Messrs. Walter G. Whitmore and Edwin R. Pearson, class of 1887, Massachusetts Institute of Technology, who took as the subject of their graduating thesis "A Study of the Efficiency of Incandescent Lamps." The method used is substantially like that of the Franklin Institute, but I have changed and improved the method of plotting so that the five plots tell the distribution of light, shape of filament, and its position in regard to photometer, etc. This method of testing lamps is in regular use in the laboratory, with very satisfactory results.

The standard of light used is a Methven screen which has been standardized by comparison with many standard candles at different times and by different observers, and also with a Harcourt standard burner. The mean of a large number of observations agrees so closely with the maker's value of two candles, that this is assumed to be right within the limits of error of the photometer, and no correction is used.

The photometer is a modified Bunsen, 100 inches long, graduated into inches and tenths by a dividing engine, and having also a scale of "ratios" which, when multiplied by the candle-power of the standard, give candle-powers directly. Both the standard burner and the lamp under test are thoroughly screened, so that little if any diffused light

[1] Reprinted from the *Electrical Engineer*, New York, January and February, 1890.

reaches the eye of the observer. A key near the observer's hand and a lamp overhead furnish light when needed for reading.

The lamp-holder is quite different from that used by the Franklin Institute, and will hold any of the lamps now in the market without danger of straining. The socket with lamp under test is screwed on the end of a brass rod, which is free to slide or rotate in a sleeve fixed in the centre of and perpendicular to a disk of brass. Set screws are provided for clamping the rod in the sleeve. The disk is arranged on the bottom of a U-shaped strap in such a position that it can be turned, while the rod always remains parallel to the sides of the U and in the same plane. The disk has notches every five degrees, and the strap carries a locking pawl, which also serves as an index.

The U-shaped strap is pivoted near the top in a frame, so as to have rotation about a horizontal axis; it also has a notched disk, and the frame a pawl and index. One arm of the U is longer than the other, and carries a movable counterweight.

This apparatus is set with the horizontal axis perpendicular to the line of the photometer and on a level with the centre of the greased disk; the vertical circle being adjusted to read 0° when the lamp stands vertical with the socket underneath, then the horizontal disk is placed at 0° and the rod carrying the lamp moved till the plane of the shanks is perpendicular to the line of the photometer and the horizontal axis cuts the filament midway of its length. This position of the lamp is called the "standard position," and the photometer reading for this position the "standard reading." By means of the horizontal circle any desired degree of longitude can be instantly turned towards the photometer, and, by the vertical circle, any degree of latitude. Flexible wires connected directly to the socket are used for current leads, and another set is used for measuring the potential at the lamp terminals. It is assumed that the socket and the lamp terminals will be of the same potential, except with very low resistance lamps, or with lamps requiring a large current.

Electrical measurements were made by a set of carefully calibrated Thomson's current and potential galvanometers, placed in a room free from magnetic disturbances, where the value of the earth's component had been determined. As the current taken by the potential galvanometer bears a very sensible ratio to that in the lamp, the instrument is always kept in circuit and a correction is made for the current passing through it. An adjustable resistance in the lamp circuit enabled the observer to keep the potential at the right amount. The potential galvanometer used was adjusted for Rayleigh volts, and as the difference

between this and Legal volts is nearly within the limits of accuracy of the instrument no correction for reduction to the latter was made. It was assumed that the voltage marked on the lamps was in Legal volts, and that if the lamp changed its resistance while under test the volts must remain constant, and the current and candle-power allowed to vary. If any lamps were marked in terms of the B. A. volts, then they were tested at too high a voltage.

When the lamp had been placed in the "standard position" and the potential adjusted, candle-power readings were taken at 65 different positions; the value used for each position being the mean of three or four settings of the disk, according to the method used by the observer.

The standard direction, to which all others are referred, is a line perpendicular to the plane of the shanks of the filament and lying in the equator of the lamp. By equator is understood the line cut upon the glass globe of the lamp, when upright, by a horizontal plane half-way up the filament. Zero point, or 0°, is the starting point for measuring the angular rotation of the lamp, and is at the intersection of the standard direction and the equator.

Horizontal distribution is measured by candle-power readings at each 30° around the equator, beginning and ending with 0°, or the "standard direction." Of these 13 c. p. readings, the first and last should, of course, check.

Mean horizontal candle-power is the mean of the first 12 of these readings, the check reading being omitted.

Vertical distribution is measured on each of four complete meridians, i. e. those of 0°, 45°, 90°, and 135°, on the equator. On each of these four circles candle-power readings are taken at every 30° (of latitude), beginning and ending with 0°, thus giving 13 readings, the first and last of which should check.

Standard reading is the mean of all readings in the direction of the zero point ("standard position"). It is an arbitrary position selected for convenience. By it and the "reduction factors" the candle-power (spherical or horizontal) may be determined under any given potential or current, at any subsequent time, by one reading. Great care should be taken in placing a lamp in this position, because in some lamps a very slight error would make a very considerable difference in the candle-power, and of course in the "factors." When a number of tests are to be made on any one type of lamp, it is preferable to select a point on the horizontal circle in that part of the curve where the rate of change of candle-power is very small, and so avoid error due to hasty or imperfect setting of the lamp for the standard reading.

Mean spherical candle-power is computed from 38 readings, selected from those described, with the aim of obtaining the average intensity of light in all directions. These readings are as follows: —

Mean of 4 north pole readings (longitude 0°, 45°, 90°, 135°) counts . .							1
Longitude	0°,	Latitude 60°,	120°,	240°,	300°,	giving	4
"	90°,	" 60°,	120°,	240°,	300°,	"	4
"	0°,	" 30°,	150°,	210°,	330°,	"	4
"	45°,	" 30°,	150°,	210°,	330°,	"	4
"	90°,	" 30°,	150°,	210°,	330°,	"	4
"	135°,	" 30°,	150°,	210°,	330°,	"	4
Equator, 12 readings (or mean horizontal × 12) counts							12
South pole reading assumed zero and counts							1
Total							38

The values for these positions are selected, added, and divided by 38.

Horizontal reduction factor is the mean horizontal candle-power divided by the standard reading.

Spherical reduction factor is the mean spherical candle-power divided by the standard reading.

The mean spherical candle-power divided by the mean horizontal candle-power gives a very useful factor, which is not so much affected as the two above by a slight error in setting the lamp in the exact standard position. Inspection of the curve for the 70-volt Weston lamp will show that a very slight error would materially change both the horizontal and the spherical reduction factors, but would not alter the ratio of the two. Obviously the product of the standard reading taken at any time and the reduction factors will give the horizontal or spherical candle-power without any other measurements.

The method of plotting the results of a test on a lamp is thought to be an improvement on the Franklin Institute method, and is designed to show at a glance, —

1. The distribution of light in five great circles. 2. The relative position of the lamp filament to the photometer at the zero of each circle. 3. The appearance of the filament as seen from the photometer disk at the zero of each circle. 4. The rated candle-power. 5. The mean spherical candle-power. 6. The mean horizontal candle-power. 7. A table of data.

For example, take the 70-volt Weston lamp. This lamp has a zigzag filament of rectangular section, twisted when mounted, which gives a very characteristic set of distribution curves.

On the horizontal plot we see the photometer bar represented by the black line on the left, and in the centre is the horizontal projection of the filament at the zero position, " with the plane of the shanks perpen-

dicular to the line of the photometer bar," and also a drawing of the filament as seen from the photometer disk at the zero of the circle. The curve shows that the light is greatest at about 150° and 330°, and weakest at about 60° and 240°; now, according to the Philadelphia method, we could not tell why this is so, but a glance at the projected filament (and also at the pictures of the filament seen at 0°, 45°, 90°, and 135°) shows that in one case the sides and in the other the edges of the corrugations correspond with the highest and lowest photometer readings. This plot also shows that the mean spherical candle-power is nearly equal to the mean horizontal, but that neither is up to the rating of 16 c. p. under any reasonable assumption that can be made as to the meaning of that term. The observed candle-powers are indicated by the figures written near the intersection of the curve and the radii, drawn 30° apart.

The next plot shows the distribution of light in a vertical plane passing through the zero of the horizontal circle. As before, the horizontal projection of the filament is seen in its position relative to the photometer bar at the zero of the circle, and a drawing of the filament as seen from the photometer at the zero position.

The next plot shows the distribution in a vertical plane passing through 45° on the horizontal circle. The projection of the filament shows that the divided plate and lamp was turned 45° right-handedly, thus bringing the 45° position on the horizontal plot towards the photometer. The diminished amount of light is due, as indicated by the sketch of the filament, to the presentation of the edges of the carbon to the photometer. The next two plots are taken in a similar way at 90° and 135° respectively on the horizontal circle.

The pictures of the filament were obtained by holding the lamp behind a plate of glass and before a bright lamp and tracing the shadow of the filament. The scale of plotting is, unless otherwise stated, 0.1 inch per candle, and the filaments are drawn "life size."

All of these lamps were bought, in the winter of 1886–87, of supply companies in Boston, out of stock, and presumably could not in any way be picked lamps.

The Edison lamps had the copper-plated shanks, and are now (1889) known as the "old lamp." The Weston 110-volt lamp is not now in the market, and I believe was on sale for only a short time after these were bought, an improved lamp taking its place. I do not know whether the 70-volt Weston lamp of the kind tested is in the market or not. The Thomson-Houston and Swan lamps appear similar to those now in the market, and are, as far as I know, the same.

RESULTS CALCULATED BY WM. L. PUFFER, FROM THE OBSERVATIONS OF MESSRS. WHITMORE AND PEARSON, CLASS OF '87.

Lamp. — 1887.	Mean standard reading.	Mean spherical c. p.	Spherical reduction factor.	Mean horizontal c. p.	Horizontal reduction factor.	Spherical ÷ horizontal.	Volts.	Amperes.	Watts.	Watts per spherical c. p.	Watts per hor. c. p.	Lamps per electrical horse-power.	Mean spher. cand. per electric'l horse-p.	Mean hor. can. per electric'l horse-p.
Weston 70-volt, 16 c. p.	16.63	14.25	0.857	15.13	0.909	0.942	69.96	1.021	71.43	5.01	4.72	10.4	149	158
Weston 110-volt, 16 c. p.	8.97	10.96	1.222	13.37	1.491	0.820	110.1	0.530	58.36	5.32	4.36	12.8	140	171
Thomson-Houston 52.6-volt, 20 c. p.	21.00	16.82	0.801	20.68	0.985	0.813	52.61	1.250	65.76	3.91	3.18	11.3	191	235
Thomson-Houston 75-volt, 16 c. p.	16.00	13.31	0.832	16.18	1.011	0.823	74.98	0.705	52.87	3.97	3.27	14.1	188	228
Thomson-Houston 110-volt, 16 c. p.	13.00	10.50	0.808	12.86	0.990	0.816	100.1	0.472	51.96	4.95	4.04	14.4	151	185
Edison 101-volt, 10 c. p.	10.50	8.25	0.786	10.38	0.987	0.795	100.9	0.512	51.65	6.26	4.98	14.4	119	150
Edison 101-volt, 16 c. p.	13.00	13.25	1.020	16.46	1.27	0.805	100.9	0.789	79.58	6.00	4.84	9.4	124	154
Edison 101-volt, 32 c. p.	26.53	28.59	1.078	34.21	1.290	0.836	100.9	1.295	130.7	4.57	3.82	5.7	163	195
Swan 36-volt, 8 c. p.	8.09	6.61	0.817	7.76	0.959	0.852	35.96	0.786	28.26	4.27	3.64	26.4	175	205
Swan 38-volt, 16 c. p.	13.10	10.57	0.807	12.25	0.937	0.863	38.05	1.300	49.65	4.70	4.05	15.0	179	203
Swan 46-volt, 8 c. p.	7.39	5.89	0.797	6.70	0.907	0.879	45.99	0.534	24.56	4.17	3.67	30.4	210	244
Swan 46-volt, 16 c. p.	15.54	14.38	0.925	16.73	1.077	0.859	45.99	1.113	51.18	3.56	3.06	14.6	141	177
Swan 92-volt, 10 c. p.	8.88	7.17	0.807	9.03	1.027	0.794	91.98	0.413	38.00	5.30	4.21	19.6	157	191
Swan 92-volt, 16 c. p.	15.30	11.49	0.751	14.00	0.915	0.821	91.98	0.594	54.64	4.76	3.90	13.7	185	217
Swan 92-volt, 32 c. p.	33.57	27.74	0.826	32.48	0.968	0.854	91.98	1.215	111.8	4.03	3.44	6.7	159	184

1887. LAMPS IN ORDER OF EFFICIENCIES EXPRESSED IN TERMS OF WATTS PER HORIZONTAL CANDLE-POWER.

	Lamp.	Voltage.	Rated candle-power.	Watts per candle-power.	Candles per electrical horse-power.
1	Swan	46	16	3.06	244
2	Thomson-Houston	52.6	20	3.18	235
3	" "	75	16	3.27	228
4	Swan	92	32	3.44	217
5	"	36	8	3.64	205
6	"	46	8	3.67	203
7	Edison	101	32	3.82	195
8	Swan	92	16	3.90	190
9	Thomson-Houston	110	16	4.04	185
10	Swan	38	16	4.05	184
11	"	92	10	4.21	177
12	Weston	110	16	4.36	171
13	"	70	16	4.72	158
14	Edison	101	16	4.84	154
15	"	101	10	4.98	150

1887. LAMPS IN ORDER OF EFFICIENCIES EXPRESSED IN TERMS OF WATTS PER SPHERICAL CANDLE-POWER.

	Lamp.	Voltage.	Rated candle-power.	Watts per spherical candle-power.	Candles per electrical horse-power.
1	Swan	46	16	3.56	209
2	Thomson-Houston	52.6	20	3.91	191
3	" "	75	16	3.97	188
4	Swan	92	32	4.03	185
5	"	46	8	4.17	179
6	"	36	8	4.27	175
7	Edison	101	32	4.57	163
8	Swan	38	16	4.70	159
9	"	92	16	4.76	157
10	Thomson-Houston	110	16	4.95	151
11	Weston	70	16	5.01	149
12	Swan	92	10	5.30	141
13	Weston	110	16	5.32	140
14	Edison	101	16	6.00	124
15	"	101	10	6.26	119

1887 LAMPS. TABLE SHOWING CANDLE-POWER ACTUALLY GIVEN OUT ON THE HORIZONTAL CIRCLE IN PERCENTAGE OF THE RATED CANDLE-POWER.

	Lamp.	Voltage.	Rated candle-power.	Per cent.
1	Edison	101	32	107
2	Swan	46	16	105
3	Edison	101	10	104
4	Thomson-Houston	52.6	20	103
5	Edison	101	16	103
6	Swan	92	32	102
7	Thomson-Houston	75	16	101
8	Swan	36	8	97
9	Weston	70	16	94.6
10	Swan	92	10	90.3
11	Swan	92	16	87.5
12	Weston	110	16	86.3
13	Swan	46	8	83.8
14	Thomson-Houston	110	16	80.4
15	Swan	38	16	76.6

1887 LAMPS. TABLE SHOWING CANDLE-POWER ACTUALLY GIVEN OUT IN SPHERICAL CANDLE-POWER IN PERCENTAGE OF RATED CANDLE-POWER.

	Lamp.	Voltage.	Rated candle-power.	Per cent.
1	Swan	46	16	89.9
2	Edison	101	32	89.4
3	Weston	70	16	89.1
4	Swan	92	32	86.7
5	Thomson-Houston	52.6	20	84.0
6	" "	75	16	83.2
7	Edison	101	16	82.9
8	Swan	36	8	82.6
9	Edison	101	10	82.5
10	Swan	46	8	73.6
11	"	92	16	71.8
12	"	92	10	71.7
13	Weston	110	16	68.5
14	Swan	38	16	66.1
15	Thomson-Houston	110	16	65.6

COMPARISON OF LAST FOUR TABLES, SHOWING POSITION OF LAMPS WHEN COMPARED ACCORDING TO THE METHODS.

Lamp.	Voltage.	Rated candle-power.	Watts per spherical c. p.	Watts per horizontal c. p.	Per cent spherical c. p.	Per cent horizontal c. p.
Swan	46	16	1	1	1	2
Thomson-Houston . . .	52.6	20	2	2	5	4
" " . . .	75	16	3	3	6	7
Swan	92	32	4	4	4	6
"	46	8	5	6	10	12
"	36	8	6	5	8	8
Edison	101	32	7	7	2	1
Swan	38	16	8	10	14	15
"	92	16	9	8	11	11
Thomson-Houston . . .	110	16	10	9	15	14
Weston	70	16	11	13	3	9
Swan	92	10	12	11	12	10
Weston	110	16	13	12	13	13
Edison	101	16	14	14	7	5
"	101	10	15	9	9	3

A study of these tables and plots will make it very evident that there is no commonly accepted value of a candle-power, or, if there is, that different makers vary widely in their attempt to produce it. The question, "What is meant by a 16 c. p. incandescent lamp?" has not, I believe, been settled by the courts, and until it is there will be endless dispute as to whether a given lamp is "up to candle-power or not."

For example, take the Edison 101-volt 16 c. p. lamp; if we rate it by the light given off "on the face of the loop," i. e. the standard reading, it is evidently 13.00 c. p., but if we select a point 45° from this it is about 19.00 c. p.; if we try to do better and take the mean horizontal candle-power then it is 16.46 c. p., or if we wish to take the mean spherical candle-power we find it to be 13.25 c. p. The watts in all cases being 79.58, we can sell this lamp as either 7.71, 4.09, 4.84, or 6.00 watts per candle.

It is, I think, commonly understood that the lamps of the Edison company are rated according to the following definition of a 16 c. p. lamp. A new 16 c. p. lamp will, when run at the number of Legal volts indicated by the number on its base, give out light which, at a point on the horizontal circle 45° from the shanks, will be equal to 16 standard candles. This one point is taken to represent the mean horizontal

candle-power. It will be noticed that the Edison lamps tested come fairly close to this rating, and also that the other lamps do not, indicating that the other makers do not understand a 16 c. p. lamp to be the same thing that the Edison company do.

It would be a great advantage to all parties if some authoritative statement of what is meant by the candle-power of an incandescent lamp could be made, and thus settle at once the uncertainty now existing.

ROGERS LABORATORY OF PHYSICS,
Mass. Inst. of Technology, December, 1889.

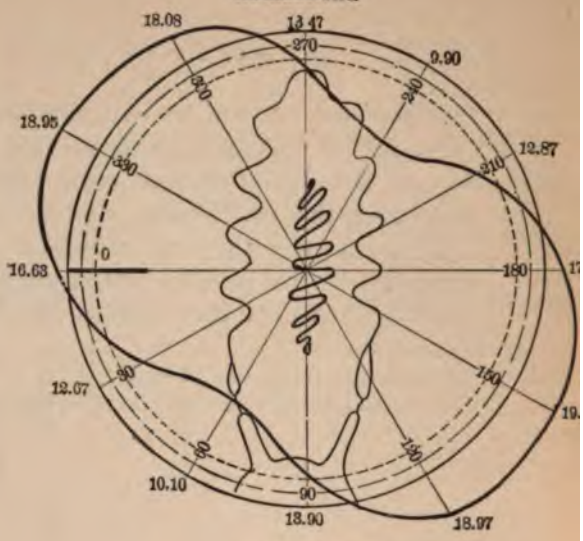
Plate I. — Data and Plots of Various Incandescent Lamps.

Lamp. — Weston, 70 volt. 16 c. p.
Mean standard reading = 16.63.
Mean spherical c. p. = 14.25.
Spherical reduction factor = 0.857.
Mean horizontal c. p. = 15.13.
Horizontal reduction factor = 0.909.
Mean sph. c. p. ÷ mean hor. c. p. = 0.942.
Volts = 69.96.
Amperes = 1.021.
Watts = 71.43.
Watts per mean sph. c. p. = 5.01.
Watts per mean hor. c. p. = 4.72.
Rated c. p. = full circle.
Mean sph. c. p. = dotted circle.
Mean hor. c. p. = dashed circle.
Section of filament = ▬
Scale, 10 c. p. per inch.

W. L. P.

Full circle reduced from 3.2″ to 2.5″ in engraving.

HORIZONTAL

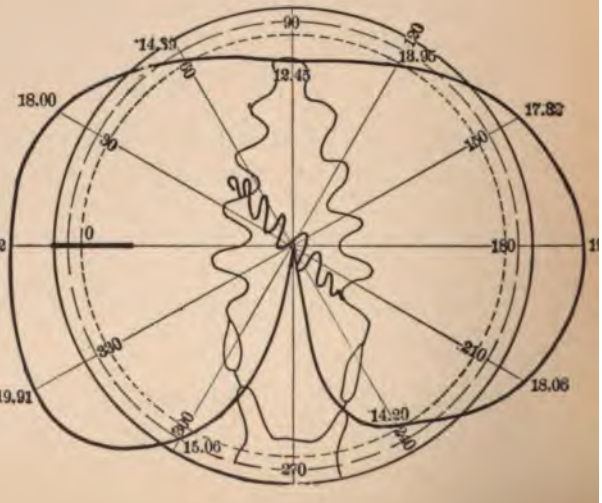


VERTICAL 0°

VERTICAL 45°

VERTICAL 90°

VERTICAL 135°



Lamp. — Weston, 110 volt. 16 c. p.
Mean standard reading = 8.97.
Mean spherical c. p. = 10.96.
Spherical reduction factor = 1.222.
Mean horizontal c. p. = 13.37.
Horizontal reduction factor = 1.491.
Mean sph. c. p. ÷ mean hor. c. p. = 0.820.
Volts = 110.1.
Amperes = 0.530.
Watts = 58.36.
Watts per mean sph. c. p. = 5.32.
Watts per mean hor. c. p. = 4.36.
Rated c. p. = full circle.
Mean sph. c. p. = dotted circle.
Mean hor. c. p. = dashed circle.
Section of filament = ▬
Scale, 10 c. p. per inch.

W. L. P.

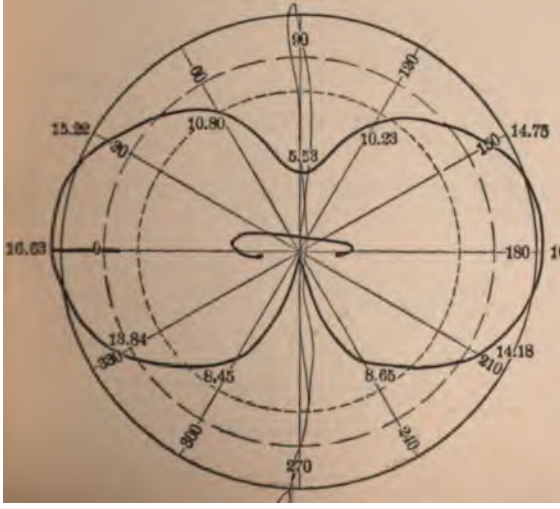
Full circle reduced from 3.2″ to 2.5″ in engraving.

HORIZONTAL

VERTICAL 0°

VERTICAL 45°

VERTICAL 90°



VERTICAL 135°

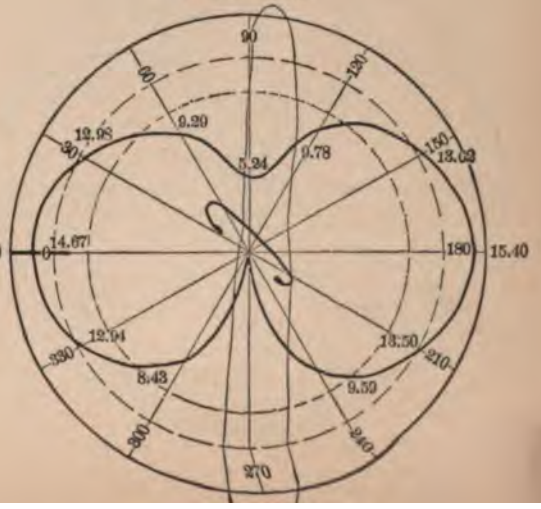


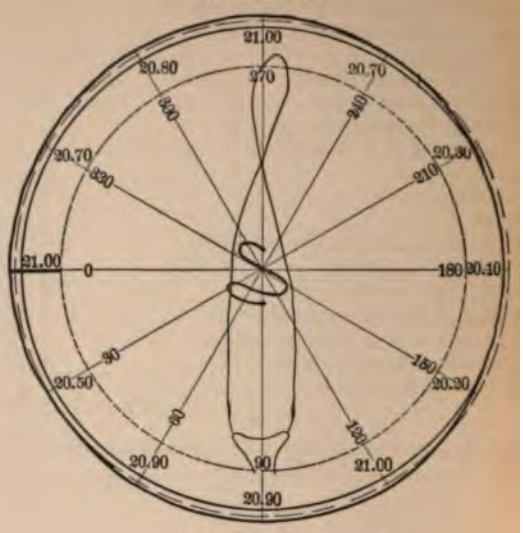
Plate III. — Data and Plots of Various Incandescent Lamps.

Lamp. — Thomson-Houston, 52.6 volt. 20 c. p.
Mean standard reading = 21.00.
Mean spherical c. p. = 16.82.
Spherical reduction factor = 0.801.
Mean horizontal c. p. = 20.68.
Horizontal reduction factor = 0.985.
Mean sph. c. p. ÷ mean hor. c. p. = 0.813.
Volts = 52.61.
Amperes = 1.250.
Watts = 65.76.
Watts per mean sph. c. p. = 3.91.
Watts per mean hor. c. p. = 3.18.
Rated c. p. = full circle.
Mean sph. c. p. = dotted circle.
Mean hor. c. p. = dashed circle.
Section of filament = ●
Scale, 10 c. p. per inch.

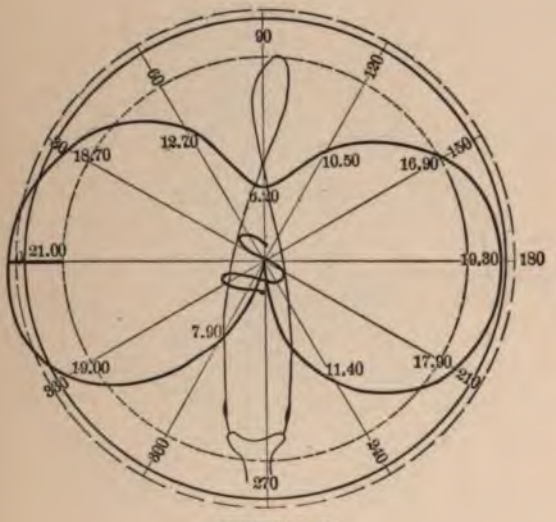
W. L. P.

Full circle reduced from 3.2″ to 2.5″ in engraving.

HORIZONTAL



VERTICAL 0°



VERTICAL 45°

VERTICAL 90°

VERTICAL 135°

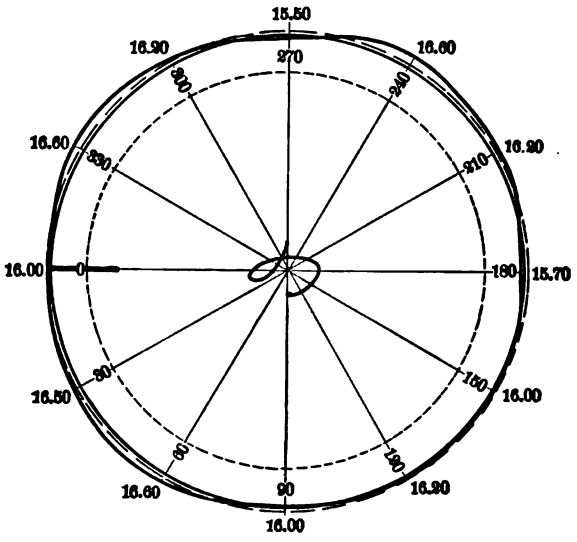
Plate IV. — Data and Plots of Various Incandescent Lamps.

Lamp. — Thomson-Houston, 75 volt. 16 c. p.
Mean standard reading = 16 00.
Mean spherical c. p. = 13.31.
Spherical reduction factor = 0.832.
Mean horizontal c. p. = 16.18.
Horizontal reduction factor = 1.011.
Mean sph. c. p. ÷ mean hor. c. p. = 0.823.
Volts = 74.98.
Amperes = 0.705.
Watts = 52.87.
Watts per mean sph. c. p. = 3.97.
Watts per mean hor. c. p. = 3.27.
Rated c. p. = full circle.
Mean sph. c. p. = dotted circle.
Mean hor. c. p. = dashed circle.
Section of filament = ●
Scale, 10 c. p. per inch.

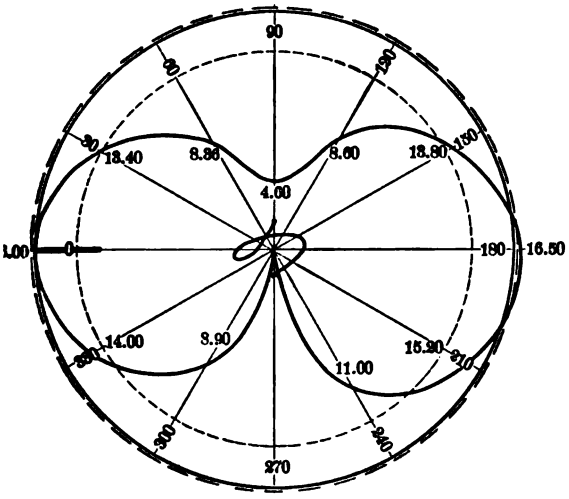
W. L. P.

Full circle reduced from 3.2″ to 2.5″ in engraving.

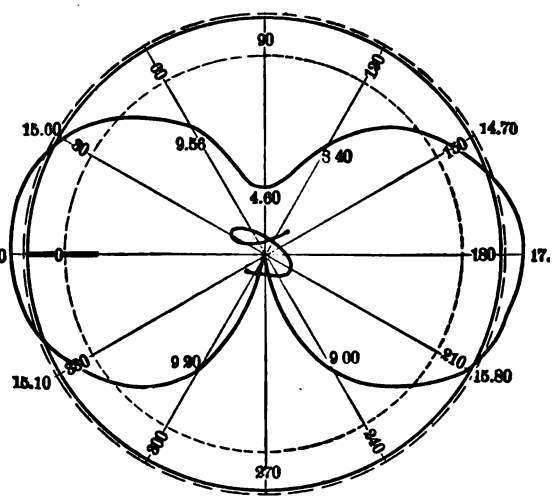
HORIZONTAL



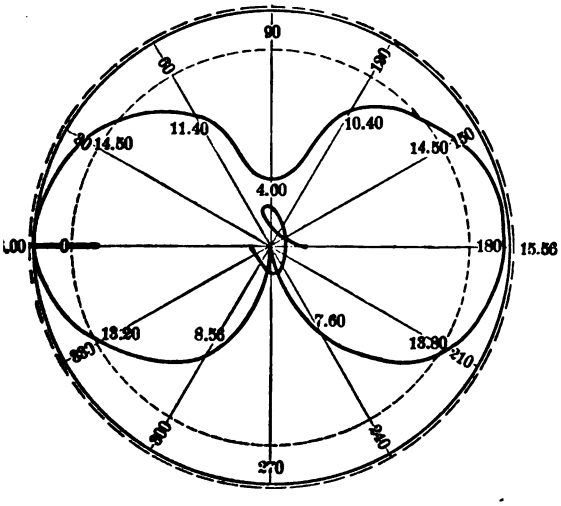
VERTICAL 0°



VERTICAL 45°



VERTICAL 90°



VERTICAL 135°

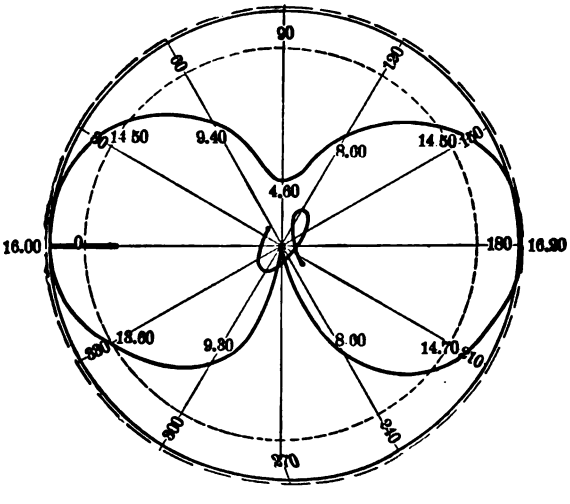


Plate V. — Data and Plots of Various Incandescent Lamps.

Lamp. — Thomson-Houston, 110 volt. 16 c. p.
Mean standard reading = 13.00.
Mean spherical c. p. = 10.50.
Spherical reduction factor = 0.808.
Mean horizontal c. p. = 12.86.
Horizontal reduction factor = 0.990.
Mean sph. c. p. ÷ mean hor. c. p. = 0.816.
Volts = 110 1.
Amperes = 0.472.

Watts = 51.96.
Watts per mean sph. c. p. = 4.95.
Watts per mean hor. c. p. = 4.04.
Rated c. p. = full circle.
Mean sph. c. p. = dotted circle.
Mean hor. c. p. = dashed circle.
Section of filament = ●
Scale, 10 c. p. per inch.

W. L. P.

Full circle reduced from 3.2″ to 2.5″ in engraving.

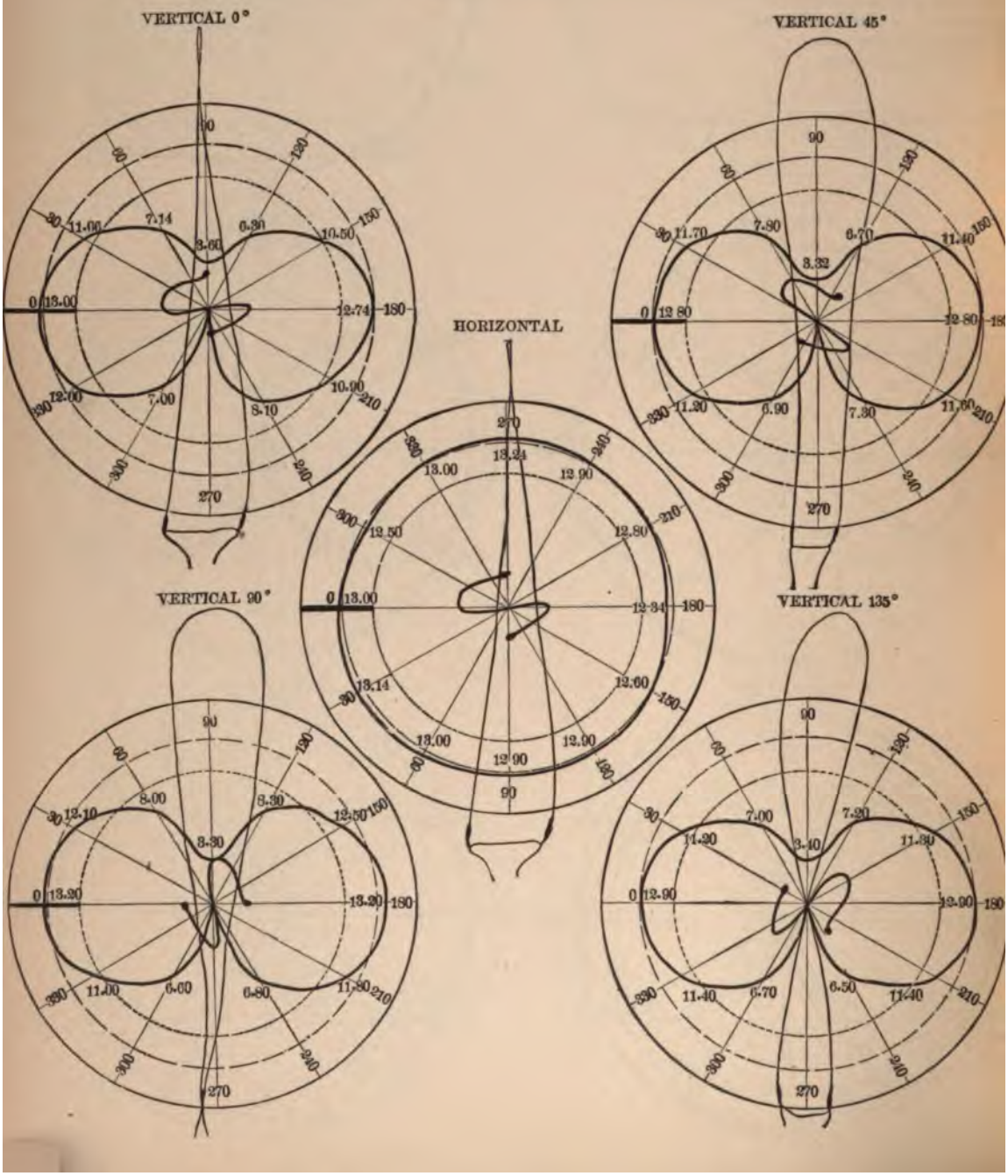


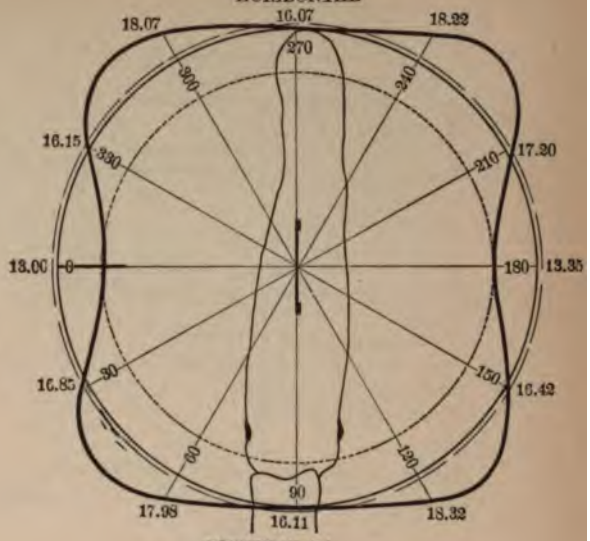
Plate VI. — Data and Plots of Various Incandescent Lamps.

mp. — Edison, 101 volt. 16 c p.
ean standard reading = 13.00.
ean spherical c. p. = 13.25.
herical reduction factor = 1.02.
ean horizontal c. p. = 16.46.
orizontal reduction factor = 1.27.
ean sph. c. p. ÷ mean hor. c. p. = 0.805.
lts = 100 9.
mperes = 0.789.
atts = 79.58.
atts per mean sph. c. p. = 6 00.
atts per mean hor. c. p. = 4.84.
ted c. p. = full circle.
ean sph c. p. = dotted circle.
ean hor. c. p. = dashed circle.
ction of filament = ■
ale, 10 c. p. per inch.

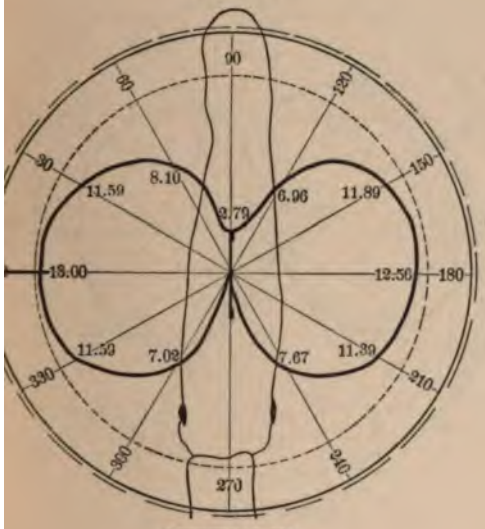
W. L. P.

ll circle reduced from 3.2″ to 2.5″ in engraving.

HORIZONTAL

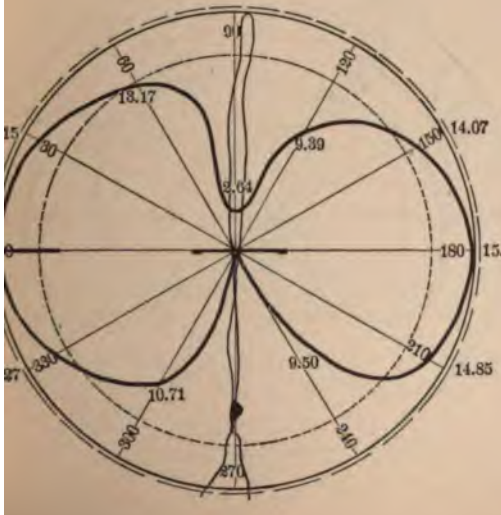


VERTICAL 0°



VERTICAL 45°

VERTICAL 90°



VERTICAL 135°

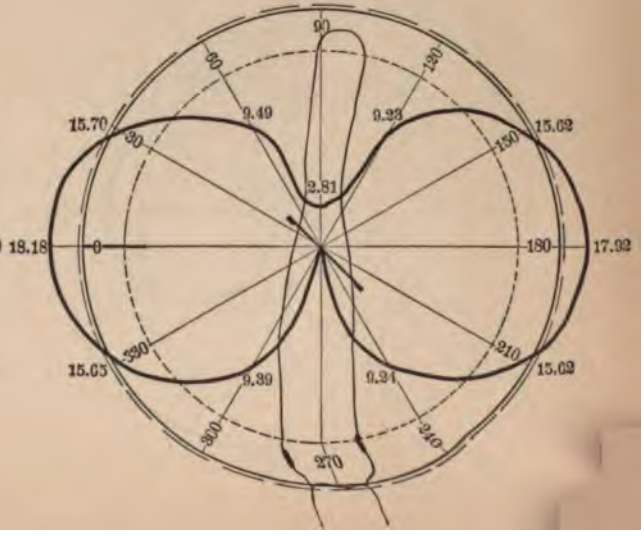


Plate VII. — Data and Plots of Various Incandescent Lamps.

Lamp. — Swan, 38 volt. 16 c. p.
Mean standard reading = 13.10.
Mean spherical c. p. = 10.57.
Spherical reduction factor = 0.807.
Mean horizontal c. p. = 12 25.
Horizontal reduction factor = 0.937.
Mean sph. c. p. ÷ mean hor. c. p. = 0.863.
Volts = 38.05.
Amperes = 1.30.
Watts = 49.65.
Watts per mean sph. c. p. = 4.70.
Watts per mean hor. c. p. = 4.05.
Rated c. p. = full circle.
Mean sph. c. p. = dotted circle.
Mean hor. c. p. = dashed circle.
Section of filament = ●
Scale, 10 c. p. per inch.

W. L. P.

Full circle reduced from 3.2″ to 2.5″ in engraving.

HORIZONTAL

VERTICAL 0°

VERTICAL 45°

VERTICAL 90°

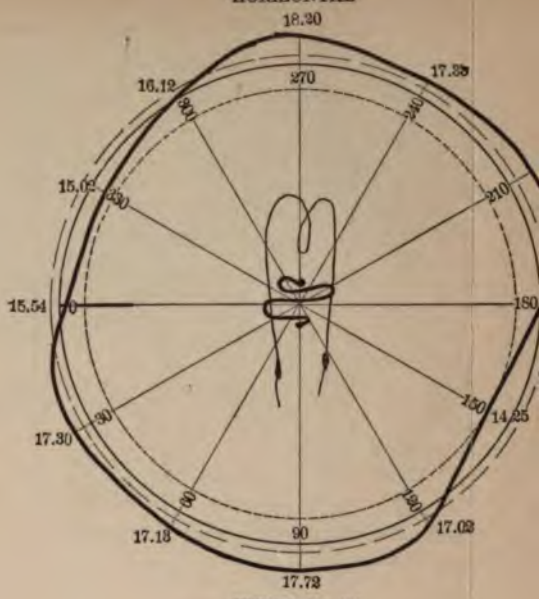
VERTICAL 135°

Lamp. — Swan, 46 volt. 16 c. p.
Mean standard reading = 15.54.
Mean spherical c. p. = 14.38.
Spherical reduction factor = 0.925.
Mean horizontal c. p. = 16.73.
Horizontal reduction factor = 1.077.
Mean sph. c. p. ÷ mean hor. c. p. = 0.859.
Volts = 45.99.
Amperes = 1.113.
Watts = 51.18.
Watts per mean sph. c. p. = 3.56.
Watts per mean hor. c. p. = 3.06.
Rated c. p. = full circle.
Mean sph. c. p. = dotted circle.
Mean hor. c.p. = dashed circle.
Section of filament = ●
Scale, 10 c. p. per inch.

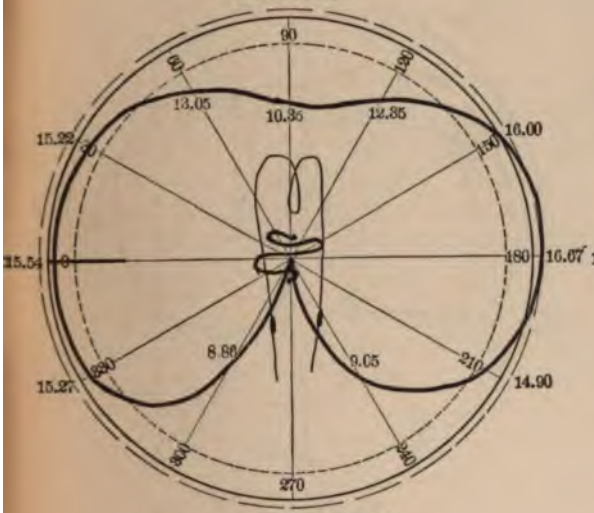
W. L. P.

Full circle reduced from 3.2″ to 2.5″ in engraving.

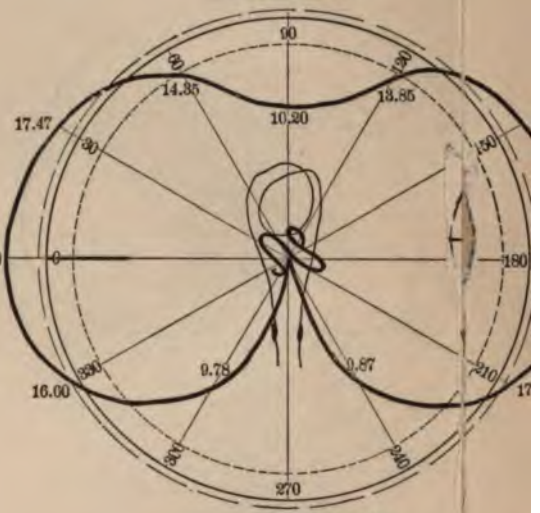
HORIZONTAL



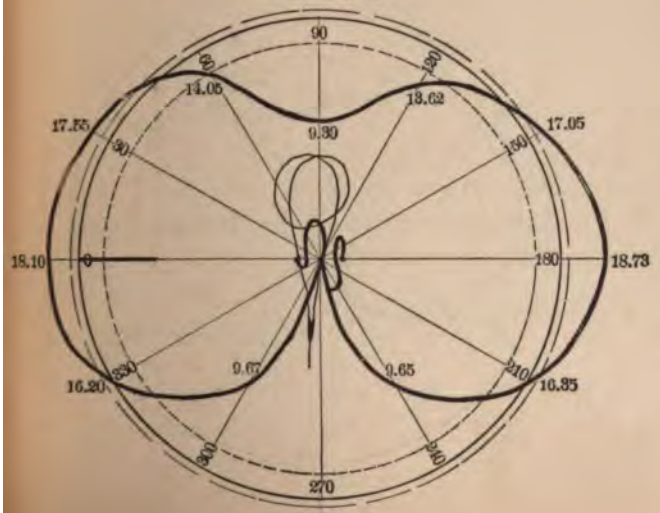
VERTICAL 0°



VERTICAL 45°



VERTICAL 90°



VERTICAL 135°

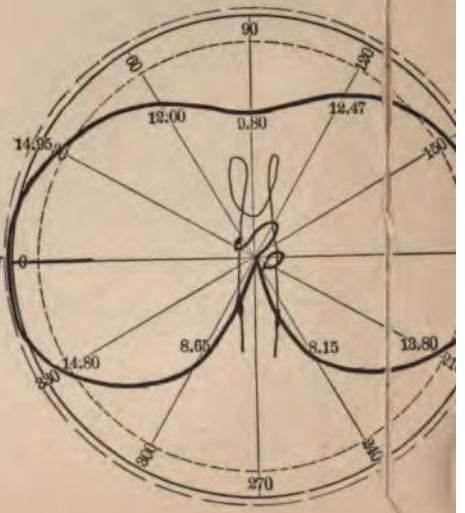


Plate IX. — Data and Plots of Various Incandescent Lamps.

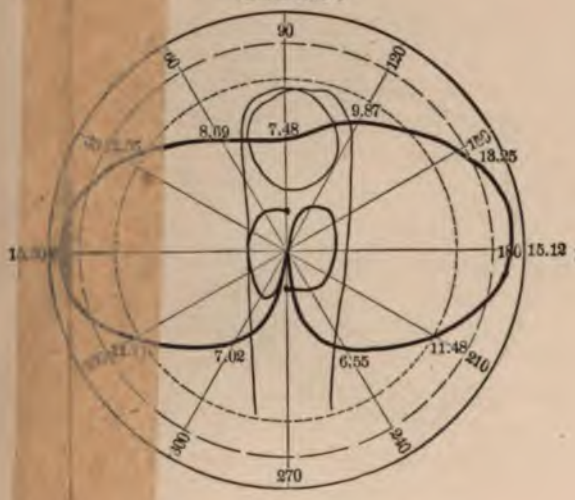
Lamp.—Swan, 92 volt. 16 c. p.
Mean standard reading = 15.30.
Mean spherical c. p. = 11.49.
Spherical reduction factor = 0.751.
Mean horizontal c. p. = 14.00.
Horizontal reduction factor = 0.915.
Mean sph. c. p. ÷ mean hor. c. p. = 0.821.
Volts = 91.98.
Amperes = 0.504.
Watts = 54.64.
Watts per mean sph. c. p. = 4.76.
Watts per mean hor. c. p. = 3.90.
Ruled c. p. = full circle.
Mean sph. c. p. = dotted circle.
Mean hor. c. p. = dashed circle.
Section of filament = ●
Scale 10 c. p. per inch.

W. L. P.

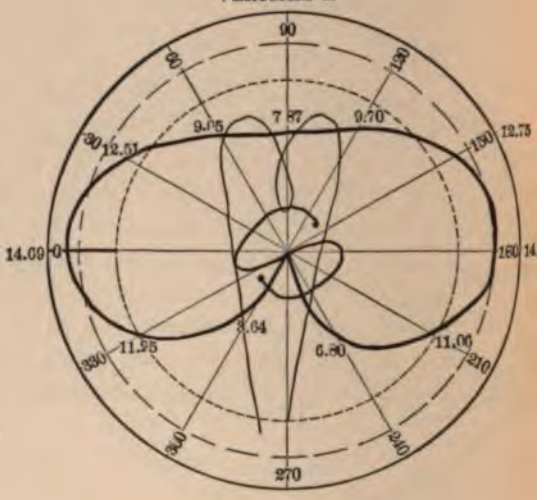
Full circle reduced from 3.2″ to 2.5″ in engraving.

HORIZONTAL

VERTICAL 0°



VERTICAL 45°



VERTICAL 90°

VERTICAL 135°

AN EXPERIMENTAL STUDY OF THE ERRORS OF DIFFERENT TYPES OF CALORIMETERS.[1]

BY CECIL H. PEABODY, ASSOCIATE PROFESSOR OF STEAM ENGINEERING, AND ARTHUR L. WILLISTON, S. B.

THIS paper gives the results of some experiments on calorimeters for determining the moisture in steam, made by Mr. Williston in the laboratories of the Massachusetts Institute of Technology in the preparation of his graduation thesis. The object of the experiments was to determine the nature, magnitude, and causes of errors in calorimeters of different types, and to find how to avoid them, or how to correct them if unavoidable.

Four forms of calorimeters were used in the experiments:

(1) a throttling calorimeter designed by the writers;
(2) a Barrus continuous water calorimeter;
(3) a Hoadley calorimeter;
(4) a barrel calorimeter.

The throttling calorimeter consists of a chamber, into which steam is admitted through a throttle valve, and from which it escapes through a larger valve. The steam is superheated by the throttling or wire-drawing, and its condition can be entirely determined by the temperature and pressure, which are taken by a thermometer and pressure gauge. The pressure of the steam in the main steam pipe from which the sample is taken is also observed. The calorimeter and the pipe and valve leading to it are well wrapped with non-conducting material, to prevent radiation. If it be assumed that no heat is lost by the steam, the heat in one pound will be the same in the calorimeter as in the main steam pipe, and we have the equation

$$xr + q = \lambda_c + c_p(t_c - t_c), \qquad (1)$$

in which x is the part of one pound of fluid in the steam pipe that is steam, $1 - x$ being water; r is the heat of vaporization, and q the heat of the liquid at boiler pressure; λ_c is the total heat, and t_c the temperature of saturated steam at the pressure in the calorimeter; t_c is the

[1] Presented at the Twentieth Meeting of the American Society of Mechanical Engineers, New York, 1889.

temperature of the superheated steam in the calorimeter; and c_p is the specific heat of superheated steam at constant pressure (0.48 nearly). From the equation (1), the values of x and $1 - x$ are readily found by the aid of a table of the properties of saturated steam.

The Barrus calorimeter consists essentially of a straight vertical tube running through an open wooden bucket, and forming a small surface condenser. The condensed steam is collected at the bottom of the tube, cooled and weighed. The condensing water is let in so as to circulate round the tube, is then mixed and drawn off, and weighed separately. The pressure of the steam is observed, and the temperatures of the condensed water and of the cold and warm condensing water are taken. Let the weight of the condensed water be w, and of the condensing water W; let r be the heat of vaporization, and q the heat of the liquid, of steam at the pressure in the supply pipe; let q_3 be the heat of the liquid at the temperature of the water resulting from the condensation of steam, collected at the bottom of the vertical tube before it is passed through the cooler, and let q_1 and q_2 be the heats of the liquid corresponding to the initial and final temperatures of the condensing water. Then the quality of the steam, x, and the amount of priming, $1 - x$, may be calculated by the equation

$$w(xr + q - q_3) = W(q_2 - q_1) + e, \qquad (2)$$

in which e is the heat lost by external radiation, to be determined by a special experiment.

The Hoadley calorimeter is non-continuous, and consists of a surface condenser of thin copper to which steam is admitted, and in which it is collected, placed in a thin copper cylinder containing the condensing water, and thoroughly wrapped, lagged, and jacketed with eider-down and hair-felt in spaces formed by three concentric galvanized iron cylinders. Arrangements are made for stirring the condensing water and for taking the mean temperature. The calorimeter is placed on a special platform scale, so that weights may be taken of the calorimeter, (1) when empty; (2) when the cooling water is run in; (3) when steam has been admitted, condensed, and collected in the surface condenser. The condensed water may also be drawn off and weighed separately, but not satisfactorily, as more or less water will adhere to the sides of the condenser. The equation given for this case is

$$w(xr + q - q_2) = (W + W')(q_2 - q_1) + e, \qquad (3)$$

in which w and W are the weights of the condensed steam and condensing water, and W' is the water equivalent of the copper forming

the condenser and calorimeter; r is the heat of vaporization, and q the heat of the liquid corresponding to the pressure of the sample of steam; q_1 and q_2 are the initial and final temperatures of the cooling water; and e is the external radiation, while x is the quality of the steam, and $1 - x$ is the amount of priming.

The barrel calorimeter was a wooden barrel set on scales, with pipes and valves for supplying cold water and steam, and with a large valve for rapidly emptying the barrel at the end of a test. The temperature of the cold water was taken before it entered the barrel, and the correction for radiation and the water equivalent of the barrel avoided by first filling the barrel with hot water, at about the temperature of the contents of the barrel at the end of the test, and then emptying rapidly and filling with cold water for the test. The equation for this case, using the same notation as in the preceding equations, is

$$w(xr + q - q_2) = W(q_2 - q_1). \qquad (4)$$

At the suggestion of the writer, the calorimeters were supplied with superheated steam, for which the condition could be known from the temperature and pressure, provided that Regnault's value of the specific heat c_p may be assumed to be true at all temperatures and pressures. In most of these tests the steam was superheated only a small amount to avoid uncertainty from this source. If λ is the total heat and the temperature of saturated steam at the pressure in the supply pipe, and if t'_s is the temperature of the superheated steam in that pipe, then equations (1), (2), (3), and (4) become

$$\lambda + c_p(t'_s - t) = \lambda_c + c_p(t_s - t_c), \qquad (5)$$

$$w\{(\lambda - q_2) + c_p(t'_s - t)\} = W(q_2 - q_1) + e, \qquad (6)$$

$$w\{(\lambda - q_2) + c_p(t_s - t)\} = (W + W')(q_2 - q_1) + e, \qquad (7)$$

$$w\{(\lambda - q_2) + c_p(t'_s - t)\} = W(q_2 - q_1). \qquad (8)$$

1. Throttling Calorimeter.

The errors of this calorimeter are of two sorts, — those arising from inaccuracies of the thermometer or the gauge, which can be calculated, and those, like the error from radiation, which cannot be calculated.

In the description previously given, it was pointed out that small inaccuracies of thermometer or gauge had slight effect on the indications of this calorimeter. Thus, at one hundred pounds' pressure absolute, an error of one pound in pressure giving apparently one hundred and one

pounds will make the priming appear to be from 0.02 to 0.03 of one per cent larger than it really is. At fifty pounds absolute the error may be twice as much. On the other hand, an error of one pound in the reading of the calorimeter gauge may give an error of 0.2 of one per cent when the pressure is five pounds absolute, and an error of 0.025 when the pressure is thirty-five pounds absolute. An error of one degree in the thermometer may give an error of 0.025 of one per cent.

Two series of tests were made on this calorimeter, using superheated steam; the first, for which the results are given in Table I., to determine the effect of varying degrees of throttling and superheating, and the second, for which the results are given in Table II., to determine the effect of running varying amounts of steam through the calorimeter.

TABLE I. — THROTTLING CALORIMETER.

	Pressure absolute in supply pipe, pounds.	Pressure absolute in calorimeter, pounds.	Temperature F. in supply pipe.	Temperature F. in calorimeter.	Superheating in supply pipe.	Superheating in calorimeter.	Loss in thermal units per pound of steam used.
1	80.2	19.9	316.2	280.2	4.2	52.4	2.6
2	80.6	23.5	317.4	285.2	5.4	48.4	2.5
3	80.3	28.4	318.0	288 1	5.9	40.9	3.0
4	79.8	32.7	318.4	290.5	6 8	35.3	3.6
5	80.8	37.5	318.5	293.4	6.0	30 1	3.4
6	87.4	24.1	353 1	318.0	35.1	100.0	6.8
7	87.4	29.2	350.6	342.0	32 6	93.3	8.0
8	87.2	34.5	350.1	344.0	32 3	85.7	7.4
9	87.2	39.3	348.8	345.4	31.0	79.3	7.5

The first five tests were made with just enough superheating in the supply pipe to determine the quality of the steam, and with as nearly as possible the same quantity of steam flowing through the calorimeter per minute.

The last four tests were made with a considerable amount of superheating in the supply pipe, and with, also, a varying degree of throttling. The loss of heat per pound is about double that in the preceding set of tests, but this is due to the fact that less steam per minute was run through the calorimeter; the radiation, though larger, could not have been twice as great as in the preceding case. Each set of tests, considered by itself, shows that the effect of varying degrees of throttling has a very small influence on the result, the greatest variation in

the loss for either group being 1.2 thermal units, or about one tenth of one per cent.

TABLE II. — THROTTLING CALORIMETER.

	Pressure absolute in supply pipe, pounds.	Pressure absolute in calorimeter, pounds.	Temperature F. in supply pipe.	Temperature F. in calorimeter.	Superheating in supply pipe.	Superheating in calorimeter.	Weight of steam used per hour, pounds.	Loss in thermal units per pound of steam.	Loss in thermal units per hour.	Loss per hour per degree of difference of temperature between calorimeter and air.	Error in priming from loss by radiation, per cent.
	1	2	3	4	5	6	7	8	9	10	11
1	88.0	25.0	429.8	361.0	111.3	121.0	43.5	19.3	850	2.4	2.2
2	88.0	24.9	410.5	353.1	92.0	113.3	54.0	12.8	690	2.0	1.5
3	87.0	25.0	390.2	349.2	72.5	109.2	69.0	6.4	442	1.5	0.7
4	87.4	24.9	390.2	350.2	72.2	110.4	70.5	5.5	388	1.3	0.6
5	87.5	24.9	384.8	346.1	66 7	106.3	75.0	4.9	368	1.2	0.5
6	86.1	24.9	374.4	340.5	57.5	100.7	90.0	2.8	352	0.9	0.3
7	85.7	25.0	361.9	329.2	45.3	89.2	102.0	2.3	233	0.8	0.2
8	85.2	24.9	350.4	320.7	34.2	80.9	135.0	0.9	122	0.4	0.1
9	84.0	25.0	343.6	314.4	28.4	74.4	137.0	0.9	123	0.4	0.1
10	78.0	25 0	330.4	303.1	20.4	63.1	201.0	0.9	181	0.8	0.1

An examination of Table II. shows that the greatest error of this calorimeter comes from radiation, and that for small quantities of steam per hour, the error may be 2.2 per cent. But the table also shows that the error may be reduced to one tenth of one per cent by the simple expedient of running a sufficient quantity of steam per hour. For the calorimeter used, which was four inches in diameter and ten inches long, at least 120 pounds of steam per hour should be used. As the calorimeter has been made with a simple valve for admitting steam, there is no way of knowing how much steam is used except by condensing and weighing it. In the future it is proposed that these calorimeters shall be provided with rounded orifices of suitable size for giving the proper quantity of steam per hour. The diameter of the orifice may be calculated with sufficient accuracy by the following formula given by Napier:

$$G = \frac{p}{70} F,$$

in which G is the flow of steam per second for the area F in square inches, and p is the absolute pressure above the orifice in pounds per

square inch. For the calorimeter used in these tests, the orifice should be about 0.2 of an inch in diameter.

The greatest defect in this type of calorimeter has been shown to be that the steam will not be superheated by throttling if the priming is more than a certain per cent, depending on the pressure of the steam. This defect may perhaps be remedied by letting the steam flow through a priming box with provision for withdrawing and measuring the water that collects in it, from which box the steam can flow through an orifice to the superheating chamber. In such case the flow through the orifice must be determined by special experiment, as the tests on flow of steam presented at this meeting show that Napier's formula may be in error to the extent of two per cent.

2. The Barrus Calorimeter.

With this type of calorimeter accurate instruments and a very considerable degree of care and skill are required, to give good results. Thus, an error of one hundredth of a degree F. in the temperature of the cooling water, whether cold or warm, will produce an error of one thirtieth of one per cent in the result. An error of one degree in the temperature of the condensed steam will affect the result by one tenth of one per cent. It appears that a good thermometer will answer for the latter, but that for the former an exceptionally good thermometer must be used, and that changes of the freezing point are to be guarded against. An error in the steam pressure is not much more serious than for the throttling calorimeter. The greatest difficulty is in determining the weight of the condensed steam and of the cooling water. An error of one hundredth of a pound in the determination of the condensed steam will cause an error of one tenth of one per cent in the result in a test of twenty minutes' duration. A slight difference of level of the water in the glass water-gauge on the pipe draining the condensing pipe will make such a difference in weight, but this error is constant, and the per cent of error can be reduced by a longer test. Common platform scales are commonly used for weighing the condensing water, and they are liable to an error of half a pound, which for a test of twenty minutes will give an error of fifteen hundredths of a per cent in the result.

In Table III. are given the data and results of tests, twenty minutes in duration, on a calorimeter of this type when supplied with superheated steam. The small differences between the heat supplied and

the heat accounted for show that the calorimeter is capable of good work, and that it is not necessary to correct for radiation.

TABLE III. — THE BARRUS CALORIMETER.

	Boiler pressure, absolute, pounds.	Temperature F. of superheated steam.	Temperature F. of cold condensing water.	Temperature F. of warm condensing water.	Temperature F. of condensed steam.	Weight of steam, pounds.	Weight of condensing water.	Heat acquired by condensing water per pound of steam.	Heat yielded by one pound of steam.	Difference in B. T. U.	Error in priming, per cent.
1	87.1	344.3	53.26	85.58	306.9	331.5	11.72	912.7	915.1	+2.4	+0.25
2	87.5	363.4	53.53	85.81	309.8	329.25	11.56	917.6	921.4	+3.8	+0.4
3	87.7	362.8	53.28	85.77	311.0	393.5	9.82	969.6	967.8	−1.8	−0.2
4	89.3	414.1	54.83	86.86	310.8	328.5	11.114	945.2	944.4	−0.8	−0.1
5	89.0	404.2	53.38	86.98	307.7	336.13	11.27	941.9	942.8	+0.9	+0.1
6	88.3	405.5	55.38	86.81	311.7	335.25	11.17	441.2	939.6	−1.6	−0.2
7	88.8	402.1	55.19	87.03	311.5	337.0	11.36	940.8	937.9	−2.9	−0.3
8	88.6	406.2	55.24	86.96	311.9	334.25	11.21	942.4	939.6	−2.8	−0.3
9	89.1	412.9	55.26	86.79	310.7	335.3	11.12	445.4	943.9	−1.5	−0.2
10	89.0	417.0	55.44	87.12	311.9	333.5	11.11	947.7	944.7	−3.0	−0.3

3. THE HOADLEY CALORIMETER.

With this calorimeter, an error of one hundredth of a degree in the temperature of the condensing water will cause an error of 0.04 of one per cent in the result. An error of one pound in the weight of the condensing water will cause an error in the result of 0.5 of one per cent, while an error of one tenth of a pound in the weight of the condensed steam may give an error of two per cent. The difficulty and uncertainty of drawing this water off, and of weighing it separately, is the most objectionable feature of this calorimeter.

In testing this calorimeter with superheated steam, the supply was constant, and during the intervals of a series of tests, it was diverted into a condenser to dispose of it. The data and results of the tests are given in Table IV. The water equivalent of the copper forming the calorimeter and surface condenser was determined by Mr. Hoadley to be 17.2 pounds, and is included in the weights of condensing water given.

TABLE IV. — THE HOADLEY CALORIMETER.

	Pressure of steam, absolute, pounds.	Temperature F. of superheated steam.	Superheating in supply pipe, degrees F.	Temperature F. of cold condensing water.	Temperature F. of warm condensing water.	Weight of condensing water.	Weight of condensed steam.	Gain in B. T. U. per pound of condensing water.	Gain in B. T. U. by condensing water per pound of steam.	Loss in B. T. U. by each pound of steam.	Difference in B. T. U.	Error in priming, per cent.
1	72.3	337.1	32.15	54.44	78.60	225.7	4.30	22.15	1155.2	1143.8	−11.4	−1.2
2	81.2	353.8	41.0	58.32	81.65	227.0	4.60	23.29	1149.7	1147.4	− 2.3	−0.2
3	70.1	333.0	30.2	59.74	85.56	225.2	5.10	25.78	1139.5	1135.2	− 4.3	−0.5
4	84.6	345.3	29.6	58.86	84.80	224.0	5.15	25.89	1130.0	1139.5	+ 9.5	+1.0
5	76.3	344.8	36.3	62.03	79.22	227.6	3.40	17.16	1145.3	1146.0	+ 0.7	+0.1
6	77.5	348.4	38.8	58.70	85.05	224.6	5.19	26.29	1139.9	1141.8	+ 1.9	+0.2
7	76.5	346.6	37.9	59.41	83.21	226.4	4.75	23.75	1132.0	1142.9	+10.9	+1.2
8	76.0	345.7	37.4	59.81	84.47	224.5	4.80	24.72	1150.1	1141.3	− 8.8	−1.0
9	76.5	346.0	37.3	59.40	90.20	224.8	6.05	30.72	1139.8	1135.5	− 4.3	−0.5
10	78.0	350.2	40.1	59.71	80.30	227.3	5.32	26.53	1135.7	1141.3	+ 5.6	+0.6

4. BARREL CALORIMETER.

The tests given in Table V. were made on a barrel calorimeter for the sake of comparison with the tests on the calorimeters of other types. They were made with care, and may be considered to give the degree of accuracy attainable in engineering work.

TABLE V. — BARREL CALORIMETER.

	Pressure absolute in steam pipe, pounds.	Temperature F. superheated steam.	Superheating of steam in supply pipe, degrees F.	Temperature of cold water, degrees F.	Temperature of warm water, degrees F.	Weight of condensing water, pounds.	Weight of condensed steam, pounds.	Gain of B. T. U. per pound of condensing water.	Gain in B. T. U. per pound of steam by condensing water.	Loss of B. T. U. by each pound of steam.	Difference in B. T. U.	Error in priming, per cent.
1	81.0	351.9	39.2	56.50	91.50	312.6	9.5	24.61	1143.0	1136.6	− 6.4	−0.7
2	73.7	338.9	32.7	56.43	91.20	311.8	9.6	24.54	1122.8	1131.8	+ 9.0	+1.0
3	79.4	347.6	36.2	56.38	88.50	320.0	9.1	24.49	1127.4	1138.9	+11.5	+1.3
4	77.0	348.8	39.5	56.42	88.95	325.9	9.2	24.53	1146.7	1139.2	− 7.5	−0.8
5	75.5	345.2	37.4	56.45	88.05	317.3	8.7	24.56	1150.3	1137.7	−12.6	−1.4
6	76.2	347.6	39.1	56.49	90.40	324.2	9.5	24.60	1154.8	1136.1	−18.7	−2.1

THE DEVELOPMENT OF THE SYSTEM OF AMALGAMATION OF GOLD ORES AT PRESENT IN USE IN THE MINING LABORATORY OF THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

BY ROBERT H. RICHARDS, PROFESSOR OF MINING AND METALLURGY.

IN the winter of 1872–73 the little five-stamp battery, which had been obtained from the Union Iron Company of San Francisco as a part of the first equipment of the Mining Laboratory, was run on small lots of ore, severally from North Carolina, Georgia, Colorado, and New Hampshire. After each run, the plates, of ordinary sheet copper coated with mercury, were scraped to clean off the amalgam which had accumulated during the run, and were then supposed to be ready for the next lot of ore.

After the last of the runs, the plates were scraped a second time, and more gold amalgam was obtained; a third scraping still yielded gold, and it was evident that in these runs North Carolina ores had been credited with Georgia gold, and Georgia ores with New Hampshire gold.

Thus, at the outset, there loomed up the great difficulty which besets all laboratory experiments; namely, the problem of how to make careful quantitative experiments with small-sized working machines. In 1876 the first new experiment was tried. Instead of the ordinary thick copper plates, the thinnest to be found in the market, about $\frac{1}{50}$ inch, were used; after each run the entire set was dissolved in sulphuric acid, and the whole of the gold recovered. This entirely overcame the first difficulty, which may be called the *overlapping error*, but at the cost of much trouble and expense, and it led to another and possibly a worse difficulty; namely, the error due to new plates, causing a great loss of gold in the tailings, and which has been called the *new plate error*. It is well known among mill men that a new copper plate, although coated with mercury, is not in the best condition to catch gold, and that a layer of gold amalgam must be formed before the plate can be considered at all reliable. In fact, no mine is willing to send ore to a mill which has just put in new plates; so that in many places it is counted as one of the regular expenses of starting a new mill to run one hundred or more

tons of gold ore, in order to charge the plates, before taking in custom work.

After many trials the method of preparing the plates with the addition of silver amalgam was adopted. This has given means for overcoming all difficulties, and has made possible the testing of gold ores by the ton in an accurate and efficient manner. It would seem that the method as worked out in the laboratory was equally well suited to large mills, and therefore a somewhat detailed account may be of value.

Soft copper plate $\frac{1}{16}$ inch thick, of the size required, is scrubbed with sand, sal-ammoniac, quicklime, and mercury until it is plated. Dilute nitrate of mercury will plate the copper far more quickly, and with less trouble, but not so permanently, so deeply, or so thoroughly. If very obstinate spots are found, dilute nitrate of mercury may be used; and if this fails, a plumber's scraper will remove the spot. When thoroughly plated and washed clean, the plates are dried carefully with cotton batting. Mercury is then sprinkled on through chamois skin, and distributed with cotton. This will give the plates a brilliant, mirror-like surface, which will remain for several days if water is kept off them. The least addition of water will very soon bring out the yellow stain which is the enemy of the amalgamator. This yellow stain is prevented on old plates by the coating of gold amalgam which has formed on their surfaces.

This difficulty, as well as the new plate error, is overcome in the mining laboratory method by the substitution of silver amalgam for gold amalgam. Silver amalgam is made by dissolving silver in nitric acid diluted with an equal weight of water, and then adding about ten times as much quicksilver as silver. The silver is all precipitated, making a pasty amalgam; the liquor, after standing over night, is then decanted, and the remaining amalgam washed thoroughly with water. If silver dollars are used, one will make amalgam enough to paint about four square feet of surface. This amalgam is dried and squeezed in chamois skin, to remove the excess of mercury, until it is of about the consistence of soft putty. It should then be applied dry to the bright plates with a flat hog's-bristle paste-brush. The amalgamated plates should be allowed to drain for at least twelve hours, to avoid, as far as possible, the presence of free mercury on the plates during the run, as it is liable to flour and be lost.

Silver amalgam is just as good a catcher of gold as gold amalgam, so far as we yet know. This layer of silver amalgam also overcomes the overlapping error, because the gold never comes to any considerable extent into contact with the copper plate; and hence, when the

plates are scraped off in the usual way, the silver amalgam practically carries with it the whole of the gold. The first two cleanings up will be diluted with silver as a matter of course, but after this no such trouble will ensue.

The Institute mill, at the time these investigations were made, was a five-stamp battery, with stamps weighing originally 55 pounds, $3\frac{1}{2}$-inch face of shoe and die, $9\frac{1}{2}$-inch drop, 88 drops per minute. It used about 34 kilos of water per minute, and stamped through a screen punched with holes $\frac{1}{60}$ inch in diameter. The discharge was on both sides, — that is, double issue. We amalgamated in the battery without battery plates. The plates were five in number; net length available for amalgamation 5 feet 6 inches each, or a total length of $27\frac{1}{2}$ feet, and width of 10 inches. The plates sloped $1\frac{3}{8}$ inches to the foot, and never gave much trouble from settling sand. This is the minimum slope at which we should dare to place them; $1\frac{1}{2}$ inches to the foot would be a better slope. The mercury trap at the end of the plates was a kind of quicksand apparatus, which allowed the mercury to discharge below as fast as it accumulated.

When cleaning up, the battery amalgam was kept by itself, and that from each one of the five plates by itself. This enabled us to see when and where the amalgamation took place. The sand was separated by a *spitzkasten* apparatus into sand and slimes. The sand was concentrated, and yielded concentration and tailings. Assays were then made of the original ore, the residue in the battery, concentrations, tailings, and slimes.

TABULAR STATEMENT OF THE PROCESS.

Ore,	Amalgams,	Battery,	
		1st plate,	
		2d plate,	
Mercury,		3d plate,	Each parted by itself.
		4th plate,	
Silver,		5th plate,	
		Mercury trap,	
	Sand or spitzkasten,	Slimes, for chlorination or other treatment.	
		Sand on table,	Concentration for chlorination or other treatment.
			Tailings, — waste.

Pure mercury was used in the battery, and hence this lot when assayed gave us the fineness of the gold in the ore.

Besides the above tests, a sample was always taken from the pulp as it came from the stamp-mill, and also as it came from the plates. These two samples were taken at the rate of $\frac{1}{4}$ litre (dipperful) every

fifteen minutes. They served to give additional testimony in regard to the working of the plates.

Four tests were made; two of them on a low-grade ore from New Hampshire; the others on an ore from Nova Scotia, which was sent to us through the kindness of Mr. J. Fraser Torrance.

I. The New Hampshire lot. This was composed mostly of quartz, stained yellow with iron rust, having cavities from which the iron pyrites had been dissolved. It contained a small percentage of pyrites, but no gold that was visible as we examined the heap. This lot also contained some fine mica-schist, which was probably from the wall-rock.

This lot of ore weighed 2021.6 kilos (4447.5 pounds). A sample of it yielded to fire assay $4.14 in gold to the ton of 2,000 pounds, reckoning gold at $20.67 per ounce troy. The total value, therefore, of the gold in this lot was $9.20.

The yield of gold in the different parts of the process was as follows: —

2021.6 kilos ore containing		$9.200
yielded products as follows:		
Gold immediately available.		
Battery amalgam	$4.573	
1st plate "	2.694	
2d plate "	.215	
3d plate "	.180	
4th plate "	.099	
5th plate "	.050	
Mercury trap amalgam	.038	
		7.849
Gold recoverable from pyrites.		
Pyrites concentrations 74 kilos	0.421	
Residue left in battery 11.75 kilos	0.227	
		0.648
Gold lost in tailings, both the weight and the contents were obtained by difference, weight = 1935.85 kilos, containing		0.703
		$9.200

The result of this experiment may be summed up as follows: —

	Per ton.
Gold by fire assay	$4.14
Gold extracted by amalgamation	3.53

or 85.2 per cent is saved by amalgamation.

The poverty of the iron pyrites concentrations found by us corroborates the conclusions arrived at by Professor Hitchcock, who says that he has universally found the gold in these mines to be in the quartz, and not intimately associated with the pyrites, as it is in some other localities.

II. The second lot of ore was taken from the same heap as the last, but at a different time. The lot weighed $403\frac{6}{100}$ kilos (888 pounds),

and assayed $4.14 per ton. When cleaned up, the different parts yielded gold as follows: —

Battery amalgam	$1.220
1st plate	.430
2d plate	.063
3d plate	.041
4th plate	.028
5th plate	.020
Mercury trap	.020
Total yield	$1.822

or $4.10 per ton.

According to the above assays the lot yielded to fire assay $4.14, to amalgamation $4.10 per ton, or 99 per cent of the gold was saved. This, of course, we cannot claim to have done; we can only say that the method of valuing by fire assay is not fine enough to give the cents exactly on such low-grade ore.

III. The third lot of ore was from Nova Scotia, and was composed of bluish-white quartz and blue slate. It contained a very little pyrites, arsenical pyrites, and galena. The gold sights, or visible lumps of gold, had all been picked out of this lot of ore; and when it was crushed in a Blake's crusher to one cubic inch in size, it showed no gold. These sights, I should judge, may have amounted to as much as $5 or $6 for the lots Nos. III. and IV. inclusive.

This lot, No. III., weighed 639.87 kilos (1407.7 pounds). No satisfactory valuation could be obtained from it, as the gold proved to be in such coarse condition. A sample of 48.45 kilos (106.6 pounds), when crushed and passed through a sieve having twelve holes to the linear inch, yielded: —

	Per ton.
Gold pellets on sieve	$7.10
Siftings, by fire assay	7.23
Total value	$14.33

After the ore was stamped and the mill cleaned up, the following figures were obtained from the different products.

639.87 kilos of ore containing gold		$10.0861
yielded products as follows:		
Gold immediately available.		
Battery amalgam	$9.1324	
1st and 2d plates amalgam, together	.1389	
3d plate amalgam	.0083	
4th plate "	.0188	
5th plate "	.0048	
Mercury trap	.0134	
		9.3166

Gold recoverable from pyrites.

Pyrites concentrations 9.9 kilos	$0.1124	
Battery residue 7.05 kilos	0.0481	
		$0.1605
Gold lost in tailings, both the weight and the contents obtained by difference, weight = 622.92 kilos		0.6090
		$10.0861

The amalgam from the battery was in large, coarse nuggets, attached together by liquid mercury. Scarcely any of this gold appeared to have made its way through the sieves. A very different result is shown in both the first and second lots of ore, where the gold was evidently fine, and therefore passed through quite freely, and more than one third was caught on the plates.

It should be stated here that the lot marked No. II. was run after that marked No. III. By comparing the yields of the plates of No. I., No. III., and No. II., it will be readily seen how perfectly we have overcome the overlapping error.

IV. A second portion of the above lot was stamped without the use of amalgam or amalgamated plates. This lot weighed 631.8 kilos (1390 pounds). When the battery residue was panned out, it yielded nuggets of gold valued at $8.24, or $11.85 per ton. The yields, therefore, by the above tests were: by fire assay, of doubtful accuracy, $14.24; by stamping with amalgamation, $13.24; by stamping alone through $\frac{1}{50}$-inch holes, $11.85.

It may not be amiss to sum up the principal precautions considered necessary by mill men, and confirmed by our experience.

The amalgam on the plates should not be allowed to get too hard or too soft. If too hard, it may fail to catch the gold; if too soft, mercury will flour at the lower end of the plates.

A quantity of mercury, varying from 1 to $1\frac{1}{2}$ ounces, should be fed to the stamps for every ounce of gold contained in the ore under treatment. This mercury should be fed a little at a time every half-hour. If the plates are inclined to stain, a lump of cyanide of potassium held a moment on the spot will remove the yellow stain. Too free use of the cyanide will cause the amalgam to soften too much on the plates, and cause a loss at the lower end.

Keith says, that if an ore contains ferrous or cupric sulphate (blue or green vitriol), as a result of oxidation of pyrites, and giving rise to the yellow stain on the plates, a little lime should be added along with the ore. This will precipitate the metals, forming harmless gypsum with the liberated sulphuric acid.

The use of oil about the mill should be restricted as much as possible, as this, more than anything else, causes the yellow stain to come. If the cams are oiled with an oily rag on a stick every half-hour, the excess and dripping may be largely prevented. Molasses is sometimes used to lubricate the cams, but it is a poor lubricant. It does no harm to the amalgam. The cams should be wiped clean every day.

Copper plates in the battery we are inclined to recommend strongly, although we have not used them ourselves, owing to the difficulty of making a quantitative experiment with them.

The development of this method of testing gold ores is largely due to the energy and zeal of the several assistants in the Mining Laboratory during the eight years 1872 to 1880; namely, Messrs. William Foster, E. Faunce, B. P. Tilden, and R. W. Lodge, who, together with the students of the successive classes, spared no pains or trouble to make the laboratory methods a success.

THE FISHERY QUESTION.

PART I. — THE NORTH AMERICAN FISHERIES, TO THE DISSOLUTION OF THE TREATY OF WASHINGTON.

An Abstract prepared as a Class Exercise in International Law, Fourth Year, Course IX., *by* JOHN W. LINZEE.[1]

THE fisheries of British North America have been a subject of international controversy from the commencement of the eighteenth century. Among European powers, France first availed herself of the riches to be found in the waters which washed the eastern shores of her possessions on the North American continent. France's ambition received a great check in 1745 from the conquest of Louisburg by troops composed mainly of Massachusetts men. But by the Treaty of Aix-la-Chapelle, in 1748, this fortress on Cape Breton was restored to France. War again broke out in 1755, and Louisburg was again besieged by an army, under Lord Amherst; the fortress was conquered, but the success was due in large measure to the fact that nearly one third of the effective men of Massachusetts were engaged in this expedition. By the Treaty of Utrecht, in 1713, France had given up Newfoundland and Nova Scotia. By the Treaty of Paris, in 1763, she gave up all her possessions on the St. Lawrence and by the sea, retaining only St. Pierre and Miquelon. To these islets she has clung with great pertinacity. The privileges she obtained by the Treaty of Utrecht, of participating in the fisheries of Newfoundland, and of frequenting a limited range of coast for the purpose of prosecuting these fisheries, remain in force to the present day, with few modifications.

No longer disturbed by the French fisherman, the colonists of New England prosecuted the fisheries with vigor and energy, but the industry was destined to receive a fatal blow from the home government. Parliament enforced the Navigation Laws, which effectually checked the export trade. The West India products were made dutiable in colonial ports, and the French again obtained virtual control of the fisheries. But by evasions of the law, Massachusetts merchants were able, notwithstanding Parliamentary action, to keep the industry alive. The course of legislation from the accession of

[1] For list of authorities consulted, see page 60.

the Stuarts down to the Revolution was directed towards restraining and breaking down the commerce of the Colonies. Duties were imposed on rum, molasses, and sugar imported into the Colonies from West India islands other than British. This act was designed to destroy the colonial trade with the French, Dutch, and Spanish islands, whereby the products of the islands were exchanged for fish. In 1775 the final blow came. Parliament determined to starve the colonists into submission. A bill was agreed to, to restrain the trade and commerce of the New England Provinces to England, Ireland, and the British islands in the West Indies, and to prevent any fisheries on the Banks of Newfoundland. Furthermore, encouragement should be given the fisheries of Great Britain and Ireland at America's expense. This aroused the colonists, and thus we see how large a place in their estimation the fisheries occupied, and how the latter figured among the causes which led to the Revolution.

The treaty of September 3, 1783, by which our independence was acknowledged, conceded to the people of the United States "the liberty to enjoy unmolested the right to take fish of every kind on the Grand Bank and on all the other Banks of Newfoundland, also in the Gulf of St. Lawrence, and at all other places in the sea where the inhabitants of both countries used at any time to fish heretofore; and also that the inhabitants of the United States shall have liberty to take fish of every kind on such part of the coast of Newfoundland as British fishermen shall use (but not to dry or cure the same on that island); and also on the coasts, bays, and creeks, and all other of His Majesty's dominions in America; and that the American fishermen shall have liberty to dry and cure fish in any of the unsettled bays, harbors, and creeks of Nova Scotia, Magdalen Islands, and Labrador, so long as the same shall remain unsettled; but so soon as the same, or either of them, shall be settled, it shall not be lawful for the said fishermen to dry or cure fish at such settlement without a previous agreement for that purpose with the inhabitants, proprietors, or possessors of the ground."

Between the signing of this treaty and the war of 1812, the British population along the shores of New Brunswick and Nova Scotia increased, and their fishing interests, enjoyed in common with the Americans, also greatly increased. Great Britain considered that, in accordance with the rules of international law, the liberty to fish granted by the treaty of 1783 naturally terminated by the war, and refused to grant Americans, gratuitously, the privilege they formerly enjoyed, and at the same time declined to consider the claim set up

by the United States government of "an immemorial and prospective right to the fisheries." No understanding was reached during the negotiations which ended in the Treaty of Ghent, in 1814, because of the untenable demands of the American Commissioners; and the officers of the English fleet in British waters were ordered not to interfere with American fishermen on the Banks of Newfoundland, in the Gulf of St. Lawrence, or on the high seas, but to exclude them from the harbors, bays, and creeks of all his Majesty's possessions. Some Americans who trespassed in British waters were seized, and the United States was finally forced to settle amicably a question which might at any moment lead to difficulties between the two countries. The result was a Convention between England and the United States, in which the rights of the two nations were clearly defined. These were, that the United States should have forever, in common with British subjects, the right to take and cure fish on certain parts of Newfoundland and Labrador, and on the coast of the Magdalen Islands, under a few restrictions respecting which no serious differences had arisen. The United States renounced forever the liberty formerly enjoyed or claimed by her inhabitants to take, dry, or cure fish on or within three marine miles of any of the coasts, bays, creeks, or harbors of British America, not included within the limits before mentioned, and the whole controversy in past years between England (representing the Dominion of Canada and Newfoundland) and the United States has been on the true effect of the renunciation, on the part of the latter, of this liberty. To the above, conditions were added, that American fishermen shall be permitted to enter such bays, etc., for the purpose of shelter and of repairing damages therein, of purchasing wood and of obtaining water, and for no other purpose whatever. The proviso as at first drafted contained, in addition to the privileges of obtaining shelter, wood, and water, the words "and bait." "This the American Commissioners consented to omit, in order to obtain as great an area for in-shore fishing as possible, thus sacrificing what has proved of greater importance." The treaty was made especially with reference to cod-fishing. It was concluded in the year 1818. American fishermen frequented the Grand Banks, and entered the bays and harbors of Newfoundland for repairs and bait, and questions have arisen as to the value of these privileges within her territorial limits. Other fishing grounds are the Bay of Chaleur, the coast of Cape Breton and of Prince Edward Island, and the Bay of Fundy; the last being, with the Gulf of St. Lawrence, the fishing ground so much valued by the fishermen of New England.

"Trouble soon grew out of the different interpretations of the treaty of 1818. American vessels were seized while engaged in fishing in the Bay of Fundy when more than three marine miles from shore. The United States government understood the phrase 'three marine miles from the coast' to mean from the coast following all sinuosities. But the English government claimed that it was from a line drawn from headland to headland."[1] The latter view, as advocated by most European authorities on international law, was unpalatable to us, since it shut Americans out from the Bays of Chaleur and Fundy and the Gulf of St. Lawrence, where the most important fishing exists. The well known authorities, Wheaton and Chancellor Kent, do not support the contention of the United States.

The American view was first advanced in 1824, when the United States complained of interference with their fishing rights. The seizure of vessels continued until, on the 10th of March, 1845, Lord Aberdeen announced to Mr. Everett, who was our Envoy to the Court of St. James, that, while the British government did not admit the American construction or abandon its own, yet, out of considerations of courtesy, it would grant to American fishermen the right to fish in the Bay of Fundy "outside of three marine miles." This Mr. Everett refused to accept, as it would place his government in a false position to take as a mere favor that for which they had so long contended as due them from the Convention.

For more than six years our diplomatic correspondence does not mention the fisheries. But between the years 1847 and 1851 overtures were made to the United States for a free interchange of natural products "of the British Colonies and the United States by treaty stipulation or legislation." Canada passed an act having this object in view, in 1847, to become operative when the United States should adopt a similar measure. But Congress refused to pass such a law, although persistently urged at three successive sessions. These propositions were closely followed by others from the British Minister at Washington for reciprocal trade, either through negotiations or mutual legislation. The President in 1851 informed Congress of the terms offered by her Britannic Majesty's government, which were, if the United States would admit "all fish, either cured or fresh, imported from the British North American possessions in vessels of any nation or description, free of duty, and upon terms in all respects of equality with fish imported by citizens of the United States," her Majesty's government would "throw open to the fishermen of the United States

[1] Elliott, p. 61.

the fisheries in the waters of the British North American Colonies, with permission to those fishermen to land on the coasts of those Colonies for the purpose of drying their nets and curing their fish, provided that, in so doing, they do not interfere with the owners of private property or with the operations of British fishermen." Meanwhile the Provinces had sent several fast-sailing vessels to protect the fisheries, while her Majesty's ministers promised a small naval force. It seemed from the above proposals and display that England intended to compel the United States to legislate under duress. To this our country was not likely to submit. Besides, the treaty was now thirty years old, and it recognized clearly the right of Americans to fish at a distance of three miles from any shore. Mr. Rusk argued that the object of the naval force was to bring about a reciprocity treaty, and that the domineering spirit of England ought to be met promptly. His resolution passed the Senate on the 23d of July, and a frigate was sent to protect American fishermen under the Convention of 1818.

In 1852, a commission sat in London to settle various claims, among which was one for indemnity for the seizure of the "Washington" in the Bay of Fundy. The commissioners disagreed, but the umpire, Mr. Joshua Bates, a member of the banking-house of Baring Brothers, decided that, since the Bay of Fundy was bounded by American territory at its mouth, it was not a British bay, within the meaning of the treaty, and that it was open to the American fishermen. This was an important gain for us.

"The Canadian fishermen found it difficult to compete against the American system of bounties, and the right to a free participation in the American market became an object of prime importance. Modifications in the United States revenue laws were opposed, on the ground that Canadian fish already monopolized the export trade," but in January, 1853, the United States consented to negotiate on the subject of their commercial relations with the British Empire; and Canadians were anxious that these deliberations should secure more extended markets for the productions of British America.

The negotiations resulted in the Reciprocity Treaty of 1854, drawn up at Washington by Lord Elgin and Secretary Marcy. By this treaty American fishermen gained fishing rights analogous to those they enjoyed under the treaty of 1783, while the Canadians obtained a market for their natural products free of duty. United States fishermen could fish on Canadian coasts without any restrictions as to distance; also certain natural products of the two countries, like fish, coal, meal, lumber, and salt, were allowed to enter into each free of

duty. This treaty seems to have been of benefit to both countries. It settled an ever present cause of irritation, and opened a large and increasing market for the export trade of British North America, while Americans prosecuted one of their great industries to great advantage. From an English point of view, this treaty was considered to have given the United States additional buyers for their products and manufactures. Besides, the maritime Provinces considered the concessions made to them inadequate compensation for the fisheries of British North America; for the Americans, by their greater enterprise and the superiority of their vessels, practically beat the British fishermen in their own waters, and derived advantages beyond any granted by the United States. Again, from the national point of view of an Englishman, this treaty had its dangers, in enticing thousands of enterprising youths of the Provinces into the fishing fleet of New England, and in making the commerce of the maritime Provinces largely dependent on the United States. In those days the Provinces were isolated from one another, and a political union between them seemed still far off. While the treaty lasted, the balance of trade with respect to manufactured goods amounted to $18,000,000 in favor of the United States.

From the standpoint of the United States, American fishermen were not satisfied with thus contributing towards building up the business of their competitors at the expense of their own interests; for it was asserted that the Canadian fishery interests now had a period of unexampled prosperity. It soon became evident that the loss of revenue from the remission of duty on Canadian importations far exceeded the value of the fishing rights conceded to American fishermen. It was contended that the Canadian fishermen, by reason of their proximity to the fishing ground and the cheapness of labor and material for building boats, were enabled to compete with the American fishermen to such an extent as to render their business unprofitable. This treaty terminated in March, 1865, in pursuance of notice given by the United States one year before. The action of the Washington government was hastened by the desire that generally prevailed in the United States to punish the Provinces for the sympathy the majority of them were believed to have extended to the South during the War of Secession.

The Convention of 1818 was again in force, but the British government did not insist on its construction of that treaty. The Canadian government now began to issue licenses permitting American fishermen to use the in-shore waters. But the importance of the in-shore

fishing to us had so decreased, that, after a trial of three years, it was announced that licenses would be stopped, the plan proving a failure. At about this period, 1868, several Provinces were consolidated into the Dominion of Canada, in consequence of which the Canadian Parliament acquired jurisdiction over the sea-coast fisheries. An act was passed by this Parliament in 1868, the principal provisions of which were : —

1. The government may grant licenses to fish within three miles of the coast. 2. Any one of a number of specified officers may go on board of any vessel within any harbor of Canada, or hovering within three marine miles of any of the coasts, bays, creeks, or harbors in Canada, and stay on board as long as she may remain within such place or distance. 3. If such vessel shall remain in such harbors for twenty-four hours after she has received notice to quit, the officer may bring her into port and search her cargo; also the master shall forfeit $400, if he answer wrongly questions put to him; also, if he be found fishing within such limits without a license, the vessel, stores, and cargo shall be forfeited.

The Treaty of Washington.

"On the 8th of January, 1870, the Governor of Canada issued an order, that henceforth all foreign fishermen shall be prevented from fishing in the Canadian waters." This was considered such a gross violation of the treaty then in force that the Secretary of State requested its modification from the British Minister. These negotiations resulted in the Treaty of Washington, in 1871. This treaty revived the reciprocity treaty of 1854, but British fishermen were allowed to fish in American waters down to the thirty-ninth parallel, and it was stipulated that "fish oil and fish of all kinds being the produce of the fisheries of the United States, or of the Dominion of Canada, or of Prince Edward Island, shall be admitted into each country, respectively, free of duty." "During the negotiations that led to the Treaty of Washington, the United States offered one million of dollars for the inshore fisheries in perpetuity, not because they were of that value, but in order to avoid future inconvenience and annoyance." The British government, on their side, asserted that the privileges granted to the citizens of the United States were of greater value than those accorded to the citizens of Great Britain; hence it was provided in the treaty that a Commission should be appointed to determine the value of these additional privileges. The treaty stated that one of the Com-

missioners should be appointed by the President of the United States, one by her Britannic Majesty, and the third conjointly, and in case the third Commissioner should not have been named within a period of three months from the date when the treaty took effect, then the third Commissioner should be named by the representative at London of his Majesty the Emperor of Austria and King of Hungary. This latter clause showed carelessness on the part of the United States, for only a few years previously a member of the house of Habsburg had lost his life in Mexico through the support given the government of President Juarez by the United States. Great Britain recognized this mistake, and was swift to take advantage of it. "She determined from the first that no third Commissioner should be chosen within the three months, and so resorted to all plausible means to delay the negotiations," and she succeeded. For at the appointed time the Austrian Minister appointed M. Delfosse, the Belgian Minister, who of course was well inclined to England, since Belgium owed its political existence to Great Britain. The Commissioners met at Halifax, and the United States was adjudged to pay $5,500,000 for the use of the fishing privileges for twelve years.

There were two peculiar features about this award, viz.: — 1. That the award was made by two Commissioners, when it should have been unanimous. 2. That the amount was exorbitant. "The customs receipts for the four full years from 1873 to 1877 showed that the United States had remitted duties on fish amounting to three hundred and fifty thousand dollars a year, and when that was added to the award it was equivalent to almost ten million dollars for the use of the in-shore fisheries for twelve years, while they were not worth more than twenty-five thousand dollars a year." Notwithstanding these facts, the Senate Committee on Foreign Relations recommended the payment of the award, if Great Britain was willing to accept it. In the interval between the signing of the treaty and the payment of the award, new methods of catching mackerel had still further reduced the value to us of the Canadian in-shore fisheries. Consequently, it is not to be wondered at that the treaty gradually became unpalatable to the United States, so that, in pursuance of instructions from Congress, the President gave the required notice of the desire of the United States to terminate the Fishery Articles of the Treaty of Washington, which subsequently came to an end on the 1st of July, 1885.

The Convention of 1818 should have once more governed the relations of the two countries, but the termination of the treaty fell in the midst of the fishing season, so that the treaty remained in force

during the remainder of the season, on the understanding that the President should recommend to Congress, at its next session, a joint Commission by the two governments.

The following diagram gives an idea of the rights and liberties to which citizens of the United States are entitled in the northeastern fisheries under the

TREATY OF 1818.[1]

- **Rights of American Fishermen.**
  - **Of Fishing.**
    - In the deep sea.
    - **In Territorial Waters.**
      1. On the southern coast of Newfoundland, from Cape Ray to the Rameau Islands.
      2. On the western and northern coasts of Newfoundland, from Cape Ray to the Quirpon Islands.
      3. On the southern shores of the Magdalen Islands.
      4. On the coast, bays, harbors, and creeks, from Mount Joly on the southern coast of Labrador to and through the Strait of Belleisle, and thence northwardly indefinitely along the coast.
  - **Other than Fishing.**
    1. To dry and cure fish in any of the unsettled bays, harbors, and creeks of the southern part of the coast of Newfoundland described, and of the coast of Labrador, while unsettled, after which the consent of the inhabitants, proprietors, or possessors of the ground must be obtained.
    2. To enter all bays and harbors for the purpose of shelter, repairing damages, and procuring wood and water.
- **Rights renounced.** Any liberty heretofore enjoyed or claimed to take, dry, or cure fish on or within three marine miles of any of the coasts, bays, creeks, or harbors not included in the above mentioned limits.
- **Rights claimed but denied by Canada.**
  1. To purchase bait and other supplies.
  2. To land and trans-ship fish.
  3. To enter bays and harbors to prepare fish, and to clean and pack fish.
  4. The commercial privileges of traders.
  5. To navigate the Gut of Canso.

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[1] Elliott, p. 104.

SOME POINTS IN THE DETERMINATION OF SILICA IN SILICATES BY FUSION WITH ALKALINE CARBONATES.

BY JAMES P. GILBERT, S. B.

THE usual method of determining silica in silicates — that of fusion with the alkaline carbonates, decomposition with hydrochloric acid, and subsequent evaporation to dryness — does not always give satisfactory results. This method is universally described in the text-books as being accurate, with the occasional caution that the filtrate should also be examined for any silica which may not have been rendered insoluble by evaporation to dryness. But one looks in vain for any intimation of what may be the amount of the error, in the determination of silica as usually carried out, if one neglects to examine the filtrate. There is no doubt that many chemists have had trouble with this method, and some have given it up, preferring to volatilize the silica with hydrofluoric and sulphuric acids, and, after a careful analysis of the residue, to calculate the silica by difference. A recent writer[1] makes the statement that he has never been able to obtain more than 97.5 per cent of the total silica in highly siliceous bodies by the fusion method. The following work was undertaken in the hope of discovering some of the sources of error in the fusion process.

The influence of the temperature at which the silica is dehydrated was first investigated. Most authorities say that evaporation on the water bath to complete dryness, or until no more hydrochloric acid is given off, is sufficient to dehydrate the silica, but a few recommend subsequent heating in an air-chamber to a temperature of 110° C. A higher temperature than 110° C. is said by Fresenius to be inadmissible, since there is liability of the silica recombining with the bases present to form silicates partially decomposable with hydrochloric acid.

To test the influence of temperature on the amount of silica obtained, the following determinations of silica were made in a blast-furnace slag containing about 46 per cent of lime, 10 per cent of alumina, and less than one per cent of magnesia. The silica was dehydrated at various

[1] George Craig: Chem. News, Vol. LX. No. 1563.

temperatures, but in all other respects the process was carried out exactly as Fresenius directs. What is generally called silica, that is, the mass insoluble in hot hydrochloric acid after evaporation to dryness, is given in Column I. of the following table. This was treated with hydrofluoric acid and the weight of the residue is given in Column II. In the filtrate from the silica the alumina was precipitated by ammonia, dried, ignited, and then fused with potassium bisulphate. The residue left on treating the fused mass with water, (and correcting for the impurities of the bisulphate used,) is given in Column III. The total silica in Column IV. is calculated by subtracting the weight of the residue left after treatment with hydrofluoric acid from the weight of the silica as given in Column I., and adding that found with the alumina in Column III.

TABLE I.—ANALYSES OF BLAST-FURNACE SLAG (CONTAINING 46 PER CENT LIME) BY FUSION WITH ALKALINE CARBONATES.

	I. Residue insoluble in hydrochloric acid after evaporation to dryness.	II. Weight of residue obtained from treating Column I. with hydrofluoric acid.	III. Weight of Silica found in Alumina.	IV. Total Silica.		I. Residue insoluble in hydrochloric acid after evaporation to dryness.	II. Weight of residue obtained from treating Column I. with hydrofluoric acid.	III. Weight of Silica found in Alumina.	IV. Total Silica.
	Per cent.	Grams.	Grams.	Per cent.		Per cent.	Grams.	Grams.	Per cent.
1	41.25	.0003	.0006	41.26	9	41.33	.0013	.0005	41.25
2	41.34	.0024	.0008	41.17	10	41.71	.0036	.0007	41.42
3	41.39	.0012	.0009	41.33	11	41.53	.0028	.0007	41.27
4	41.32	.0020	.0003	41.18	12	41.89	.0031	.0004	41.35
5	41.22	.0012	.0009	41.19	13	41.79	.0032	.0002	41.41
6	41.43	.0021	.0007	41.23	14	41.65	.0027	.0005	41.43
7	41.40	.0014	.0009	41.32	15	41.90	.0028	.0001	41.48
8	41.48	.0017	.0008	41.35					

Nos. 1 to 3 inclusive were dehydrated on the water bath.

Nos. 4 to 6 inclusive were dehydrated at 125° C.

Nos. 7 to 9 inclusive were dehydrated on iron plate over Bunsen burner.

Nos. 10 to 15 inclusive were dehydrated in air-chamber at 280° C.

All evaporations were made in porcelain.

In this series of results it will be seen that the silica was practically all rendered insoluble even at the temperature of the water bath, and that there was no gain in this respect in using a higher temperature. But the amount of foreign matter in the silica was, in general, perceptibly higher at the higher temperature of dehydration. This is probably due to the alumina being rendered partially insoluble in acid. From the fact that very little silica was found in the alumina, it would seem as if there were no tendency for the silica to recombine with the lime and alumina even at a temperature of 280° C. Nos. 13, 14, and 15 were digested, after dehydration, with hot acid for one hour, in the expectation of dissolving out all the alumina and other bases, but this treatment did not give smaller residues, after the volatilization of the silica with hydrofluoric acid, than in Nos. 10, 11, and 12, which were treated only thirty minutes with acid.

The general uniformity of the results in this series suggested the possible influence of the calcium chloride present in dehydrating the silica on evaporation to dryness. This led to the second point con-

TABLE II. — ANALYSES OF A BLAST-FURNACE SLAG (CONTAINING 35 PER CENT LIME AND 15 PER CENT MAGNESIA).

	I. Residue insoluble in hydrochloric acid after evaporation to dryness.	II. Weight of residue obtained from treating Column I. with hydrofluoric acid.	III. Weight of Silica found in Alumina.	IV. Total Silica.		I. Residue insoluble in hydrochloric acid after evaporation to dryness.	II. Weight of residue obtained from treating Column I. with hydrofluoric acid.	III. Weight of Silica found in Alumina.	IV. Total Silica.
	Per cent.	Grams.	Grams.	Per cent.		Per cent.	Grams.	Grams.	Per cent.
1	33.65	.0032	.0032	33.61	7	33.49	.0032	.0011	33.24
2	32 90	.0014	.0025	33.18	8	33.85	.0045	.0012	33.27
3	33.46	.0013	.0013	33.29	9	33.90	.0042	.0012	33.30
4	33.71	.0027	.0004	33.40	10	33.65	.0062	.0062	33.62
5	33.42	.0021	.0008	33.24	11	33.83	.0046	.0045	33.80
6	33.75	.0031	.0004	33.36	12	33.95	.0088	.0050	33.60

Nos. 1 to 3 inclusive were dehydrated on the water bath.

Nos. 4 to 6 inclusive were dehydrated at 120° C.

Nos. 7 to 9 inclusive were dehydrated on iron plate, over Bunsen burner.

Nos. 10 to 12 inclusive were dehydrated at 280° C.

sidered, namely, the influence of the salts present on the determination of silica. It is not unreasonable to suppose that some salts, such as calcium chloride, may have a beneficial influence, while others, as, for example, magnesium chloride, which is decomposed when its solution is evaporated, may vitiate the silica determination by forming a silicate which will be subsequently decomposed by hydrochloric acid with solution of the silica. To test the effect of magnesium chloride determinations of silica were made in a slag containing about 35 per cent of lime, 15 per cent of magnesia, and 15 per cent of alumina. (See Table II.)

This series differs from the first mainly in the large amount of impurity in the silica and the large amount of silica in the alumina when the dehydration is effected at a temperature of 280° C. The best results were obtained at a temperature of 120° C. The increase of silica in the alumina as the temperature is increased would seem to be due to the magnesia, for the conditions were, in all other respects, the same as in the analyses of the other slag. The above results indicate that there is no danger of the silica combining with the magnesia at 120° C., and that at this temperature the silica is almost completely dehydrated, the calcium chloride probably assisting in the dehydration.

A series of analyses was next made on orthoclase, practically free from lime and magnesia.

TABLE III. — ANALYSES OF A SAMPLE OF ORTHOCLASE.

	I. Residue insoluble in hydrochloric acid after evaporation to dryness.	II. Weight of residue obtained from treating Column I. with hydrofluoric acid.	III. Weight of Silica found in Alumina.	IV. Total Silica.		I. Residue insoluble in hydrochloric acid after evaporation to dryness.	II. Weight of residue obtained from treating Column I. with hydrofluoric acid.	III. Weight of Silica found in Alumina.	IV. Total Silica.
	Per cent.	Grams.	Grams.	Per cent.		Per cent.	Grams.	Grams.	Per cent.
1	64.80	.0018	.0004	64.52	5	64.98	.0050	.0019	64.45
2	64.13	.0018	.0030	64.30	6	64.62	.0035	.0020	64.36
3	64.03	.0026	.0035	64.20	7	64.83	.0068	.0030	64.45
4	64.35	.0020	.0030	64.52	8	64.97	.0083	.0039	64.35

Nos. 1 and 2 were dehydrated on the water bath.
Nos. 3 and 4 were dehydrated at 120° C.
Nos. 5 and 6 were dehydrated on iron plate over Bunsen burner.
Nos. 7 and 8 were dehydrated at 280° C.

This series differs from the previous one in that the higher temperature is not accompanied with an increased amount of silica in the filtrate. This would seem to be a confirmation of the idea that there was a recombination of the silica and the magnesia at the temperature of 280° C. in the magnesia slag; and the fact that the amounts of silica found in the alumina are (with one exception) uniformly higher than those in the lime slag indicates, I think, the beneficial effect of the calcium chloride in the dehydration of the silica.

The third point considered was the influence of temperature on the dehydration of the silica when no bases other than the alkalies were present. From the last three series of analyses it would seem that some silica always fails to be rendered insoluble by heat alone, so that, if a definite amount of pure silica were taken and subjected to alternate fusions and dehydrations, we should expect that the amount of silica obtained would diminish at each successive treatment. To ascertain if this were so, two determinations of silica were made in the lime slag, and the silica thus obtained repeatedly treated by fusion with alkaline carbonates in the usual way. The per cent of silica recovered in each case is given below. The silica obtained from the slag by the first fusion and dehydration is taken as 100 per cent.

	I. Per cent.	II. Per cent.	
1.	42.02	41.81	Silica in slag obtained by heating to 280° C.
2.	97.24	97.79	Percentage of the above obtained in four successive fusions by heating to 120° C.
3.	96.04	96.70	
4.	95.17	95.96	
5.	94.19	95.45	

The large difference between the results of the first and second fusions is probably due to impurities in the silica, which was heated to 280° C.; but the variations in the other cases can only be due to loss of silica by incomplete dehydration. Still, these are better results than Craig obtained, who does not, however, mention the temperature he used in dehydrating the silica.

The fourth point considered was the possibility of obtaining the silica in a purer state, that is to say, to decrease the amount of the residue left after treatment with hydrofluoric acid. This residue may be due to imperfect washing, or to the fact that alumina has been rendered insoluble in hydrochloric acid by prolonged heating to a high temperature. The higher residues obtained in the cases in which the silica was heated to 280° C. point to the latter possibility. But it is not improbable, as will

be mentioned later on, that under certain conditions the silica may enclose alkaline salts so that it is impossible to remove them completely by washing.[1]

In an interesting article by Lindo,[2] on the analysis of glass, I met the recommendation that the watery solution of the fusion be diluted to a very large bulk, so that on acidifying with hydrochloric acid there shall be no precipitation of silica. In this way he obtained, on evaporation, what he called "vitreous silica," in distinction from the ordinary "amorphous silica." This vitreous silica, he says, is so easily washed that time is gained in the analysis to compensate for that lost in the evaporation of the solution. Lindo takes it for granted that one cannot get all the silica by the ordinary process of dehydration, and that the last traces of it cannot be got out of the filtrate unless there is sufficient iron oxide or alumina present to effect its complete precipitation. In the analysis of glass, he added a known amount of ferric chloride to the filtrate from the alumina, and by precipitating this iron with ammonia the last traces were obtained.

The following analyses of glass were made according to his directions. The powdered glass, about one gram, was fused with five grams of sodium and potassium carbonates, and the mass treated with about four hundred cubic centimeters of boiling water until it was thoroughly disintegrated. On acidifying with hydrochloric acid, a clear solution

TABLE IV. — DETERMINATIONS OF SILICA IN GLASS. (LINDO'S METHOD.)

	I. Silica obtained after two fusions.	II. Residue from Column I. with hydrofluoric acid.	III. Silica obtained from the filtrates.	IV. Total Silica.
	Per cent.	Grams.	Grams.	Per cent.
1	73.03	.0003	.0058	73.58
2	71.81	.0008	.0125	73.06
3	72.50	.0004	.0084	73.22
4	71.96	.0000	.0051	71.43
5	71.25	.0007	.0060	71.70

[1] In this connection it is interesting to note that all the residues left on treatment with hydrofluoric acid, in the two slags analyzed, contained manganese, which may point to an insoluble compound of manganese with the alkaline salts.

[2] Chemical News, Vol. LX. No. 1546.

was obtained. This was evaporated to dryness on a water bath, heated to 125° C., and, as recommended by Lindo, the silica thus obtained again fused and treated as before. This silica obtained from the second fusion was weighed, and then treated with hydrofluoric acid. The two filtrates were combined and evaporated to dryness, heated to 125° C. for one hour, and the residue, insoluble in hydrochloric acid and water, filtered off; this is given in the third column. The first three analyses are of German "half-white" glass; the last two, of Bohemian white glass.

In this case it is seen that a considerable portion of the silica fails to be dehydrated by one evaporation and heating to 125° C. The silica obtained in this way is very compact, and the washing, as claimed by Lindo, is very easily done. The purity of the silica thus obtained (shown by the small residue left on treatment with hydrofluoric acid) is doubtless in part due to the facility with which the silica is washed; for it is conceivable that, when the silica is separated from a concentrated solution of alkaline salts, it may enclose particles of the liquid which may not be easily washed out. But this high purity, as compared with the silica obtained from the slags and the feldspar, may be due to the absence of any large amount of alumina in the glass.

To see what would be the character of the silica obtained from the first slag by large dilution of the solution of the fused mass before acidifying, the following determinations were made.

TABLE V. — AMOUNT OF FOREIGN MATTER IN SILICA OBTAINED FROM LIME SLAG. (LINDO'S METHOD.)

	I. Weight of Silica obtained from Slag.	II. Residue from Column I. with hydrofluoric acid.	
	Grams.	Grams.	
1.	.2755	.0008	Silica obtained by evaporation to dryness on water bath.
2.	.2610	.0010	
3.	.2871	.0008	Silica obtained by evaporation to dryness and heating to 125° C.
4.	.2683	.0012	
5.	.2506	.0036	Silica obtained by evaporation to dryness and heating to 280° C.
6.	.3113	.0043	

These figures would seem to justify the inference that in the presence of considerable alumina it is not possible, even by large dilution, to get as pure silica as is easily obtained when only salts of the alkalies or alkaline earths are present.

The foregoing determinations confirm the statement that the ordinary process of fusion with alkaline carbonates and evaporation to dryness

cannot always be relied on to render all the silica insoluble. No experiments were made to determine the effect of *repeated* evaporations to dryness with hydrochloric acid.

When the evaporation is carried out in the presence of free sulphuric acid the results are more satisfactory, but, as Craig says, the amount of alkaline sulphates introduced seriously interferes with the determination of the bases present.

A comparison of results obtained from a sample of quartz by evaporation with hydrochloric and sulphuric acids is given below.

I. Silica by evaporation to dryness with hydrochloric acid and heating to 120°.	II. Silica by evaporation to dryness with sulphuric acid.
Per cent.	Per cent.
98.95	99.43
98.60	99.67
98.79	99.51
	99.70

In these determinations I followed Lindo's recommendation of diluting largely before acidifying, and the residue left, on treatment with hydrofluoric acid, was very small. The first two determinations by sulphuric acid were done by decomposing the dilute watery solution of the fusion with hydrochloric acid, evaporating to dryness, and then adding strong sulphuric acid in excess and heating on an iron plate until copious fumes of sulphuric acid were given off for several minutes. The silica was in appearance the same as obtained by Lindo's method with the aid of hydrochloric acid only. But in the last two the watery solution of the fusion was decomposed directly by sulphuric acid in considerable excess, and then evaporated as before. The silica obtained in this way was very bulky and gelatinous.

These results seem to justify the recommendation that has been made; namely, that in the analysis of silicates the silica is best determined by dehydration with sulphuric acid (in cases where it is not inadmissible by the presence of lime, barium, lead, etc.), and that the bases should be determined, after the decomposition of the silicate, by hydrofluoric acid.

MASS. INSTITUTE OF TECHNOLOGY,
January, 1890.

THE DATA OF FILTRATION. — I. SOME RECENT EXPERIMENTS ON THE REMOVAL OF BACTERIA FROM DRINKING WATER BY CONTINUOUS FILTRATION THROUGH SAND.

BY WILLIAM T. SEDGWICK, ASSOCIATE PROFESSOR OF BIOLOGY.

FILTRATION on the large scale is gradually coming to be regarded by experts as the only adequate and practical safeguard against contamination, the only available remedy for pollution, of public water supplies. Most American cities have hitherto shrunk from the heavy pecuniary outlay involved in filtration, and have preferred to rely for protection simply upon the general good character of the water-shed from which their supply is taken. Experience has shown, however, that surface waters, no matter how carefully collected or stored, are apt to suffer more or less from the presence of various vegetable or animal growths, which, on decaying, support bacteria, and give rise, either directly or indirectly, to troublesome and disagreeable tastes, odors, or turbidities. Ground waters, though usually very pure when first drawn, may, if stored in open reservoirs, suffer still more severely from the algæ or other organisms which they readily support, — sometimes in such abundance as to render the water disgusting and totally unfit for drinking. Waters in this condition will sometimes recover, after a time, their original purity; but in order to make them fit for immediate use they must be filtered. Moreover, there is reason to believe that, with the increase of population, districts suitable for uncontaminated water-sheds, etc. must grow constantly more and more rare, until eventually no large city will have within reach enough of potable water to constitute, unfiltered, a pure supply. Already, indeed, the larger cities like New York and Boston are anxiously seeking to protect the purity of their respective water-sheds, always at large expense and often only after prolonged and uncertain litigation. At the same time, fresh and more numerous complaints of "fishy" or "cucumber" tastes in stored waters are constantly testifying to the difficulties involved in satisfying the inhabitants of a modern civilized city or town with water drawn directly from a reservoir teeming with organisms, dead or alive, and without any attempt at its purification.

In view of this condition of things, it is not unreasonable to suppose that purification of some sort will soon become a necessity in American water-works precisely as it has been found indispensable in the case of certain European cities, e. g. London, Berlin, and Zürich. The remedy applied in these cases is continuous filtration through sand, and it is the present opinion of experts that in filtration, either continuous or intermittent, a sufficient protection of water supplies will eventually be found. No apology need be made, therefore, for putting before American students of sanitary science some of the latest results of European experience.

The problems involved in filtration (aside from questions of cost, administration, etc.) are three-sided, viz.: (1) engineering, (2) chemical, and (3) biological. The chemical and biological data alone concern the actual purification of the water; the engineering data are more purely mechanical, and will be considered only incidentally. The present paper will deal almost exclusively with a single biological problem of fundamental importance in all filtration, — namely, *the possibility of the passage of bacteria* (*including pathogenic bacteria*) *through sand filters*, — and will be still further confined to a summary account of some recent and highly important experiments in this direction made by an eminent authority, Dr. Carl Fraenkel, upon the sand filters of Berlin, Germany.

The water supply of Berlin is in part derived from a very objectionable source, the river Spree, but is purified, before being delivered to the inhabitants, by filtration through sand. It must be understood, moreover, that these sand filters are run continuously; i. e. their surfaces are not allowed to get dry, and the flow through them is steady rather than irregular. They are therefore very unlike filters of similar material worked intermittently, — such, for example, as those which have been so successful in the Lawrence Experiment Station of the State Board of Health of Massachusetts, — and the Berlin sand filters are especially interesting as excellent examples of continuous filtration carefully studied from the biological standpoint. Sanitary engineers who desire to avail themselves of the experience of the engineers of the Berlin water-works should consult an important paper on "Water Supply, from the Hygienic and Technical Standpoint," by Piefke, engineer of the city water-works of Berlin. ("Aphorismen über Wasserversorgung, vom hygienisch-technischen Standpunkte aus bearbeitet." C. Piefke, *Zeitschrift für Hygiene*, VII., 1889, p. 115.) This paper is also a valuable biological contribution, and should be in the hands of every sanitary biologist.

The bacteriological supervision of the Berlin filters has apparently been very thorough, and has repeatedly shown so great a reduction of the numbers of bacteria in the effluent as compared with those in the unfiltered water, that the impression appears to have become general that no bacteria really pass *through* the best filters, those found in the effluent being accounted for by contamination from the air, or by errors in manipulation, experiment, etc.

The outbreak of a serious epidemic of typhoid fever in certain districts of Berlin between January and April, 1889, (some five hundred cases having been reported,) and the comparative localization of the fever in the eastern portion of the city, threw suspicion upon the water supply, and led to the inquiry, Are the sand filters a sufficient protection against the introduction of pathogenic germs from the polluted river into the service pipes, or may bacteria pass, alive and virulent, through the layers of sand?

The solution by actual experiment of this difficult, but highly important, problem was referred by Koch to his assistant, Dr. Carl Fraenkel, a biologist of the highest reputation. The work was carried on during the summer and fall of 1889, and, in a lecture before the German Society for Public Hygiene, Fraenkel has given a preliminary account of his results, which appears in the *Deutsche Medicinische Wochenschrift* for December 12, 1889. The complete account will appear hereafter, but the results already reached are important enough to receive immediate attention. (*Wird das Trinkwasser der Stadt Berlin durch die Sandfiltration mit Sicherheit von etwaigen Infectionsstoffen befreit?*—Deutsche Med. Woch., Dec. 12, 1889.)

By the cordial co-operation of the authorities of the city water-works, and especially of C. Piefke, the engineer in charge, (whose work is referred to above,) it became easy to construct filters imitating precisely those in actual use, and in general to carry on the experiments under the normal conditions. It was obviously out of the question to undertake any experiments with pathogenic germs upon the filters actually in use, and therefore one of the first steps was to construct small tanks on the same pattern. A barrel-shaped wooden tank, 2 meters high and 0.75 meter in average diameter, having at the bottom a sieve-like perforated support, was filled precisely like the large tanks in actual use; namely, with 150 mm. of stones of the size of a fist, 80 mm. of coarse gravel, 100 mm. of fine gravel, and 600 mm. of finer (*scharf*) sand. The surface was thus one meter below the top of the tank, and was provided with an overflow. Special means were provided for avoiding sudden changes in pressure, so that the flow was perfectly regular and under control.

It was considered safer to begin the experiments with a harmless bacterium, and accordingly the preliminary experiments were made with *Bacillus violaceus*. This species was chosen, not only because of the comparative ease with which it can be detected, but also because of its rarity, and of the fact that it might be supposed as likely as any to survive the passage through the sand. It was cultivated in a very weak solution (1 part of ordinary bouillon in 20 parts of distilled water) in order to avoid the objection which otherwise might have been raised, that the germs had found in the tank itself, thus fed, the means of continued existence, or even of multiplication, and so eventually had spread to the very bottom of the tank, and finally passed out. Small doses of this fluid, which had a faint bluish color, were added to the unfiltered water, from time to time, and usually at regular intervals of six hours. The supernatant water was then stirred up in order to secure an even distribution of the violet bacillus all over the top of the tank. Samples of the water standing on the surface of the sand, as well as of the effluent, were examined bacteriologically every day, and the results of a prolonged trial, lasting from June 1 to the end of July, went to establish the fact that, *contrary to the prevailing opinion, the Berlin continuous sand filters are by no means germ tight.* The least efficient periods, moreover, are those in which the filters are "young," i. e. are not yet covered on top with a layer of organic material accumulated from the water previously passed through (*Schmutzdecke*, *Deckhaut*, *Filz*), or, on the other hand, "old," i. e. so covered with organic matter "felted" together that water passes through only under the compulsion of a high pressure. It was observed, however, that after prolonged running the number passed out increased somewhat, even under no marked increase of pressure, and Fraenkel suggests that possibly some other cause than mere pressure is at work in such cases; such, for example, as the effect of the steady accumulation of food material within the tank, giving support finally to an undue number of bacteria in the lower layers.

In any case, it appears that the number of germs in the effluent is directly dependent on the number in the supernatant (unfiltered) water. Any change in the number of *B. violaceus* applied was soon followed by a corresponding change in the effluent. Moreover, the rate of flow seemed to be of the utmost significance. The filter which was run at the higher speed always showed the more bacteria, and, indeed, so uniformly, that Fraenkel declares the results proportional. Thus, with one filter running at 300 mm. speed and another at 100 mm., it appeared that three times as many blue colonies were contained in the effluent

of the former as in the effluent of the latter. He therefore lays down the rule, that *the qualitative efficiency of a continuous filter is inversely proportional to its quantitative efficiency.*

These preliminary experiments pointed clearly to the outcome of those upon pathogenic germs, although there was the possibility that these, being doubtless more delicate, might not survive in the (to them) unusual environment of a sand filter. In fact, it has been shown repeatedly, in laboratory experiments, that certain pathogenic germs speedily die out in water containing other germs. The temperature, also, was clearly against them, so that a hope might not unreasonably have been entertained that they would be killed in the filters. From the end of July to the middle of October Dr. Fraenkel experimented with the same filters as before, now using pathogenic germs. The results showed that *the germs of cholera and typhoid may also pass through the sand filters.*

The number of these germs in the effluent depends upon the number in the unfiltered water, and on the rapidity of filtration. The beginning and the end of the filtration period are especially dangerous times, because in the former case the filter has not attained its most effective condition, and in the latter the pressure, as well as perhaps the gradual charging of the whole filter (*Durchwachsen*) with live bacteria, tends to favor the discharge of the germs.

These results are directly opposed to the prevailing opinions concerning sand filters. There was, however, no good reason to doubt that the filters used were quite as effective as the actual large Berlin filters. The general belief that the Berlin continuous filters allow no germs to pass through them does not appear to have been well founded. It rested, for the most part, upon the observation that the number of germs discharged bore no apparent relation to the number applied. The former was fairly constant, the latter very variable. Hence it was inferred that the germs found in the effluent were due merely to contamination from the lower layers, or from the air. The filters themselves were regarded as "germ tight."

Fraenkel points out, however, that it is unfair, as is the custom, to compare the water of the river with that in the service pipes, both samples being taken on the same day, since the pipe water of any given day has not necessarily the slightest connection with the river water of the same day or hour. In the absence of experimental evidence it is impossible to say just how long is required for any particular portion of water to pass through the filters and reach a particular region of the city. But it is certain that the micro-organisms do not all pass on with the same velocity, nor do they keep pace with the

water in which they exist. Again, it appears that the very idea of considering the germs in the effluent as having come in part from the lower layers, which in turn were charged from above in an earlier period, is a partial and tacit admission that germs may pass through, — as it has been shown that they do in Piefke's experiments upon new or sterilized filters, — so that the period of complete exclusion is at the best but limited.

Lest any one should conclude from these experiments, that, because the filters are not absolutely germ tight, they are therefore practically worthless, Fraenkel strongly insists that such a conclusion would be very unjust and fallacious. The reduction in the number of germs by an effectively working and intelligently operated sand filter is in truth astonishing; but it is clear that any such system as is necessary for a large city requires the most skilful and intelligent supervision, if the results are to be of the best. Furthermore, since it appears from these experiments that the number of germs in the effluent depends directly upon the number applied, it becomes highly important to employ for filtration the best water available. It is not a matter of indifference whether the water to be filtered be good or bad. Only the best water is susceptible of the highest purification. The rate of filtration is also obviously extremely important, and requires constant attention. It may be urged that in these experiments the number of germs applied was very large, — much larger than would be likely to occur even in a badly polluted supply in actual service. This is certainly true, but does not forbid the possibility of such germs passing through, although it greatly reduces the probability. On the other hand, it must not be overlooked that the samples of effluent actually examined were only a very small portion of the entire effluent, so that there is a possibility of the escape from the filters of a much larger number than was found. Moreover, if it be allowed that the germs may multiply in the filter, or that they are hardy, — as increasing evidence seems to be indicating in the case of typhoid fever, — the way is at once open for the filters to become breeding places, as well as passages for germs.

The answer to the question whether or not the epidemic of typhoid fever, referred to at the outset, might possibly have come from germs which escaped detention by the sand filters, is obvious; whatever the probability, the possibility cannot be denied.

The foregoing study of the bacterial efficiency of continuous filtration of drinking water is, from the standpoint of public hygiene, of the utmost importance. For sewage disposal, however, continuous filtra-

tion fails utterly, since there is no free oxgyen in sewage, and hence no nitrification, or other purification, except the purely mechanical effect of the straining out of suspended matters, which leads to speedy stoppage of the filters. In drinking waters there is usually an abundance of free oxygen, so that continuous filtration might, theoretically, allow the complete nitrification of the suspended matters mechanically held back. That this does not take place is probably due partly to the fact that the arrival of fresh material for oxidation takes place more rapidly than its removal by nitrification, and partly because nitrification appears to require a more or less prolonged contact between bacteria and the substances to be oxidized. It appears, therefore, that in the case of drinking water rich in oxygen, as well as of sewage destitute of oxygen, the process of continuous filtration is effective chiefly in the removal of suspended matters, and of these the finest particles (bacteria) are imperfectly separated. Intermittent filtration, on the other hand, readily effects the nitrification of the organic matters in sewage, by allowing the entrance of oxygen into the sand between the flooding periods, and the purification of drinking water, by similar oxidation of the matters mechanically held back.

It is interesting to observe the striking effect of continuous filtration upon the bacteria in the water supplies of Berlin and Zürich. According to Piefke, in the paper referred to above, omitting certain extreme cases, it appears that, from July, 1885, to April, 1886, the average number of living bacteria in the unfiltered water of the river Spree was 3,609 per cubic centimeter. During the same period the effluent from the sand filters averaged only 63. Similarly the unfiltered water of Tegel Lake contained 505; the effluent, 51. In Zürich during 1886, the lake water drawn from a depth of 12 meters averaged 159. The filters were worked at a very rapid rate (7 m. per day), yet the average bacterial condition of the effluent was excellent, namely, 24 per cubic centimeter. In 1887, owing to an extension of the plant, the filters were run more slowly (at about 200 mm. per hour), and although the lake water showed a higher bacterial content (246), that of the effluent was lower than before (19). These figures show conclusively that, although far from perfect, the bacterial purification of drinking water by continuous filtration is still very great. If all the algæ, fungi, and animal forms, as well as the débris and other suspended matters, are also removed, as they appear to be, the final result of such filtration must certainly be pronounced very satisfactory.

THE LANGUAGE OF "THE LIFE OF THOMAS BECKET."

BY FRED P. EMERY, INSTRUCTOR IN ENGLISH.

A VERY popular species of composition, during the twelfth, thirteenth, and fourteenth centuries, was the collection of fabulous stories known as the *Legenda Aurea*, stories of the lives of the saints and martyrs, who performed most incomprehensible deeds of charity and devotion, who endured torture, not because they must, but because it served to manifest their goodness, and who at last died that they might be the shining lights of the little candles that enlumine this wicked world.

To say who was the favorite saint of the Middle Ages would be difficult, but one very interesting to Englishmen, because he was himself of their nationality and because he lived within the memory of their own times, was Thomas à Becket, the Archbishop of Canterbury, who was put to death by the minions of Henry II. when that monarch had expressed his displeasure with him, and who, conceived in the popular mind as suffering torment for his faith's sake and standing upon an ecclesiastical eminence, was not merely regarded as a martyr for the cause of the Church, but was canonized as a saint. Stories of his life, a mixture of authentic history and fiction, were long current, told over and again, and reduced to writing by some unknown priest or monk of the early part of the fourteenth century.

This fragment of about twenty-five hundred lines is especially interesting to the student of philology, as being written midway between the romances and chronicles of an earlier date and the new English of Chaucer.

Examined as the basis of grammatical investigation, this poem presents, as the most prominent fact to attract notice, a great difference from the language of the century previous to its composition. The introduction of French words alone is sufficient to change the whole appearance of the text from one of earlier date. Add to that the result of the constant tendency for shortening forms, the usual curtailing of the Anglo-Saxon and semi-Saxon endings, and we have the language as it exists in the early part of the fourteenth century. In the discussion of the language of this fragment, it has seemed best to

follow the ordinary methods of philological research, and to study the different parts of speech separately, and in the usual order.

As not all the composition was carefully investigated for paradigms, some of these remain uncompleted; but enough has been done, it is hoped, to show what was the general tendency of the language of this period, and what were the principal regular grammatical forms, and what were the chief exceptions to regularity.

NOUNS.

Nouns Masculine.

Sing.	N.	e	—	man (me).	Sing.	D.	e — e	man.
	G.	es	es	manes, 2376.		A.	e —	man.
Plu.	N.	es	es or is, 2418; but see 2468.			men	en.	
	G.							
	D.	es	es			men	en.	
	A.	es	es or s			men	en.	

Plurals are found in *en* of all genders, as follows: —

1885 bellen. 1951 schrewen. 2251 emeten. 2373 honden (fem.).
248 (also 2419, 2421) schuldren. 2498 churchen.

More masculine nouns end in — than in *e*, but no definite rule can be formulated, so that we can know which form to expect. Some forms worth notice are these: —

1790 rede, seems to have *e* uniformly, though from Ags. rāed, cf. 2006.

1790 govern, from the French governe. 1807 and after, sire, voc.

1867 and 1971 freond (in voc. plu.). 1813 lymemele, a noun become an adverb, cf. modern English piecemeal. 1859 al dai used adverbially; fo, n. d. a., but fou, n. and dat. plu.

2797. A substantive adj. is found in the wise, cf. 2250.

2113. Another case, god used as a noun. See also 2409 (so in Anglo-Saxon).

2226 seems to be an older form than most of the text.

2285 fader in the gen. singular, without *s*, as survival of the Anglo-Saxon.

Nouns Feminine.

	Sing.		Plur.	
N.	e	moder.	N.	es.
G.	e, or —	moder [2286?], 2489.	G.	
D.	e	moder.	D.	es.
A.	e	moder.	A.	es.

Some of the noticeable feminine forms are as follows: —

In line 1819, par charité, French words, with a French construction.

1836 and 1872, threting, dat. and acc. without the *e*.

1866 mansing, dat. without *e*, but cf. 1901 and 1873, where the acc. has *e*.

1916 loking without *e*.

1882 and '83 croiz, here plural, but certainly singular in 2103. This word seems indeclinable, so far as distinction between nom. and oblique cases is concerned.

1887, stret is noticeable in the dat., as is also in 1979 boc.

2129 soul (here acc.), Ags. sawel, usually has a final *e*. See also 2335, 2424, and others.

2129 churche (gen. sing.). See also 2296. See also 2322, of ende day, and 2323, deth unel. The churche dore, 2307, seems very probably to be a compound word.

2263, wordle, in which a common transposition occurs, where *d* and *l* change places, as is frequent in this word.

Nouns Neuter.

Sing.	*e*	—	Plu.	
N.	e	—	N.	es (or —).
G.		es.	G.	(ne 2007).
D.	e	— or e.	D.	es.
A.	e	—	A.	es (or —).

The neuter nouns ending in — are much more frequent than those ending in *e*, and very naturally. There have been noted a few interesting forms, which do not readily fall in with the paradigms. These are: —

1815, 2437, etc., *ȝer*, the regular form of the noun in all cases except the gen. of both singular and plural. In the cases instanced, it seems to be acc. plu. of extent of time. 1835 schif in dat. keeps its *e*, which is not found in the nom. 2007 see childerne as a gen. plu., the only one found. 2144 schef preserves its old regular form in the plural, Ags. scéap. 2193 here is plural. 2187 and 2225 hors seems to be plural in these cases, as being numbers.

Noun Constructions to be noticed as Uncommon.

1867 loverdes leve the King. 1876 amorwe, an adv. formed from a noun and prep., as also 1931 akneo (cf. 2370). 1924 pound (plu.). 1966 *that* folk where a preposition would be more natural; cf. *Pronouns*. 2004 mile (plu.) like hors; still heard colloquially. 2232 Seint Johnes the Baptist (cf. 1867). 2285 and 2286, fader and moder (gen.); cf. *Fem. Nouns*, and elsewhere.

Nouns Proper.

These nouns are instanced in the order in which they occur, and are useful in comparing the forms of the early fourteenth century with those of the present day.

Seint Thomas, Engelonde (also without *e*), France, Whitsonde, Rome, Everyk, Salesbure (also *y*), Londone, Jesu Crist, Canterbure (also *y*), Dovere, Sire Renald de Warenne, Sire Randolf de Brok (also Brok*e*), Gorveis, Sandwich, Thomas, Jesu Crist, Palmsonedai, Jerusalem, Pope, Cristes (gen.), Normandie, Sire Renald de Fizonis, Sire Hughe de Moreville, Sire Williem the Traci (also Williem Traci), Sire Richard the Brut, Sire Robert de Broke (also without *e*), King Henri, Fridai, Saltwode, Childerne-masse-dai-Tuesdai (also Tywesdai), Seinte Marie dai Magdalene, Seinte Marie, Seint Dionis (also Seinte), Edward Grim, Grantebrugge, Gywes, Godspel, Excestre, Seint Austines (gen.), Seint Johnes the Baptist (gen.), Argenteyme, Templers, Claundone, Scotlonde, Sire Geffrai, Britaigne, Kyng Richard, Kyng John, Sire Geffreies, Stevene, Honori, Pandulf, Water, Reyns, Sire Huberd de Brom, Norhamptone, Ponteney, Jesus.

Nouns from the French.

By the time of this composition French words were becoming common in our language, but they occurred in no such proportions as we find them in the English of to-day. The number seems very small for so late a period as this. In this list, proper nouns are not included, as they are mentioned elsewhere. The most patent French derived nouns are the following: —

1790 gouverne, 1793 onur, 1794 condut, 1795 honur, 1797 letres, 1798 sentence, 1801 digneti, 1819 charité, 1821 grace, 1838 croice, 1839 signe, baner, 1843 cri, 1846 joye, 1853 cas, 1856 semblant, 1861 pres, 1868 trespas, 1870 deserteisoun, custume, 1873 manere, 1875 respit, 1878 contrai, 1879 parosche, 1880 processioun, 1882 croiz, 1885 taperes (i. e. = tabors, which the sense seems to favor), soun, 1886 instrument, 1887 noyse, 1895 palefrai, 1899 paleys, 1903 frere, 1905 place, 1907 forme, 1908 surance, 1921 poer, 1923 cofre, 1937 despit, 1944 traitour, 1960 folie, 1961 messager, 1967 predicatiouns, 1985 clerk (also 2933 clerc), 1986 wardeyn seems to be old French (yet ward and warder are early English, from the French through the Teutonic, like *guerre*, etc.), 2014 chambre, 2016 clerkes, consail, 2018 message, 2020 comandment, 2023 baronye, cf. also 2028, 2024 chief, 2033 servage, 2048 croune, 2136 croun (= top of head), 2056 accord, 2072 fei, 2078 maners, 2085 armes (French, but armes in 2073 is English), 2087 cloistre, 2088 compli, 2093 merci, 2098 servise, 2099 castel, 2100 foles, folies, 2109 hure, 2110 mantel, vylté, 2126 oreisoun, 2128 avowes, 2137 face, 2141 Gywes, 2142 disciples, 2152 marbel (?), 2152 poynt, 2156 pavement, 2158 colour, 2159 diademe, 2161 peynt, 2185 tresorie, 2186 robberie, 2187

tresour, 2188 chartres, 2192 franchise, 2208 auter, 2230 solempneté, 2231 minstre, 2236 abyt, 2238 covele, stamyn, 2239 seculer, 2240 priveité, 2243 ese, 2248 creature, 2272 consail, 2273 angusse, 2275 pité, 2289 euchesoun, anny, 2292 penance, 2293 priveité, 2315 stat, 2318 miracle, 2333 patriarcs, 2334 virgines, 2340 court, manere, 2360 contek, 2376 tombe, 2391 ensample, 2395 meneisoun, 2399 heir, 2402 heritage, 2413 miseise, 2425 prisoun, 2428 fyne, 2431 sauter (this word, found in Anglo-Saxon, is Latin of the Second Period, not of the Third, but this form seems to be from the French), 2443 tricherie, 2450 legat, 2451 pardoun, 2455 priours, persones, 2456 barounes, 2457 serjants, squiers, 2462 presse, 2465 covent 2479 justise, 2483 Trinité, 2488 cheances, 2506 feste.

PRONOUNS.

Personal Pronouns.

Sing.	1st.	2d.	3d. M.	3d. F.	3d. N.
N.	y, ich	thu	he	her	hit
G.	mi, myn	thi, thin	his	hire	
D.	me	the	him		hit
A.	me	the	him		hit
Plu.					
N.	we	ȝe	hi (he 1872) (he 2191)		hi
G.	oure	ȝoure	here		
D.	ous	ȝou (ȝu 2105)	hem (him 1999)		hem
A.	ous	ȝou	hem		hem

The *y* and *ich* seem to be used indiscriminately, or at any rate the distinction is not a uniform one.

The dative and accusative are alike in the pronouns, showing that the distinction between them had already been broken down.

The gen. plural ends in *e* before vowels and consonants alike, but in the singular a well defined line is drawn; *mi* and *thi* are used only before consonants, while *min* and *thin* are used before vowels or the letter *h*.

The second person nom. singular in the interrogative form is *tou*, as *havestou?*

In writings of the same date as the one under discussion, we often find the tendency to a pronoun in the dative case as the apparent subject of the verb. Some instances in this work are as follows: —

1st person dative singular as subject, as, me thinketh, 1925, 2035, 2072, etc.
2d " " " " " as, the reweth (this may be acc.).
3d " " " " " as, him thoȝte, lines 1, 3, 2378, 2395, etc.
3d " " plural " " as, hem wende, 1911, 2466, etc.; hem agros, 2200.

No forms of the genitive singular of the third personal pronoun appear in this piece; the place of that form is often supplied, in the same way as in the English Bible, by the word *thereof*, meaning simply *of it*, as used in lines 2213 and 2257.

The form *thereto* occurs several times in the sense of *to it*.

Possessive Pronouns.

The possessive pronouns are merely the genitives of the personals, all the plurals ending in *e*, as does also the feminine of the third person singular. In the singular, the forms *thi* and *mi* are used before consonants, and *thin* and *myn* before vowels or *h*. The third masculine is *his*, indeclinable, as are the others. In form the vocative is like the nominative.

Intensive Pronouns.

The emphatic forms of the possessive occur in the same form as in modern English. We have in 1822 *myn owe* in the dat. sing., and in 2060 *thin owe*.

In this composition we very frequently find a simple personal pronoun in one of the oblique cases where the modern English would use an intensive. For example, *themselves* is often expressed by *hem*, as in 1802, 1955, 2189, 2195, etc.

In the singular, too, *him* dat. sing. must be translated *himself* in 1928, 2010, 2174.

The form in *silf*, however, does occur as the nom. singular *himsilf* in 2255; in *himsilve*, line 2316; the nom. sings. *thusilf* and *the silf* occur. In the nominative we also find *oure Louerd silf;* and in the neuter is *the selve thing*, line 2478.

Demonstrative.

The forms *this* and *that*, indeclinable in the singular, are found in all cases and genders of the singular. The plural *this* occurs in the nominative; *that* is not found in the plural, so its form can only be conjectured.

Relative Pronouns.

The relative pronoun of all genders is *that*, both in the singular and plural; the forms *who* and *which* do not occur in those forms. In one shape, however, we see the former; the dative both of the singular and plural has the form *wham*, as dat. sing. *of wham*, 2462, dat. plu. *of wham*, 2001.

The pronoun *ho* does occur as a relative once, but not with the ordinary relative meaning; it is in line 2222, where it means *whoso*. The relative pronoun *that* is used in 1849, where modern English would use an adjective. So too several times we find this evidence of a careless use.

Compound Relative.

The compound relative, meaning the words that we should express in modern English by *what* or *that which*, is *that*, the same form as the simple relative; it is used in all cases of the singular, and in the nominative plural; *that* is also used in line 2240, equivalent to *those who*.

Interrogative.

The interrogative pronoun occurs only in the nominative singular. It is *ho*, the antitype of our modern *who* as interrogative and relative.

Indefinite and Occasional.

Under this heading I have grouped all those pronominal forms which do not readily resolve themselves into some one of the other classes.

me or *men*, as used like the French *on*, as the subject of a verb referring to an indefinite subject. This is very common, and in line 2486 we have *me* as a neuter nominative.

Other, meaning "another," is found in the nom. and acc. sing. and plu. The dat. plural is *othere*, which form is also used as nom. plural. *Thother* is used both sing. and plu., as 1996 sing., 1835 plu. *Thothere* is also found in nom. sing. 2151.

meni on = "many a one," nom. as 1809, 1840, 2454.

non = none (nom. sing.), *one* = "alone."

echon in all cases of singular, but *echon* dat. in 2421, and nom. in 2429 and 2474.

enes is acc. sing. neuter.

nothing is found in nom., dat., and acc. singular.

alle in dat., nom., and acc. plural masculine.

summe (nom. plu. masc.).

pece and *other* = "piece by piece" (nom. sing. 2420).

bothe (nom.), *beire* (gen.).

ADJECTIVES.

The vocabulary of this author is very limited in regard to adjectives. The adjectives are many in number, and almost every noun has its accompanying epithet; but while in number they are so many, in

kind they are very few; almost every object or person mentioned is either good or bad, "holi" or "lither." The paradigms are in no case complete in this fragment.

Strong Declension.

Sing.	*Masc.*	*Fem.*	*Neut.*
N.	— 1939, 1940, 2021.	— 1789, 1998.	
G.		No genitives are found.	
D.		*e* 2083, 2089, 2087, 2166, or — 1826, 1974, 2099.	
A.	— 2034, 2444.	— 1937, 1980, 2033.	— 1988.
Plu.			
N.		*e* 2001, 2480, or — 2066 and elsewhere.	
G.		No gen. plu. is found.	
D.		*e* 2466.	
A.		— 1920 (?).	

The plural is alike for all genders.

Weak Declension.

Sing.	*Masc.*	*Fem.*	*Neut.*
N.	*e* 1848, 2086, or — 1897, 2097	Lacking.	
G.	2069 (holi) 2376.		
D.		(owe) 2062, 2429.	*e* 1894, or — 2483.
A.	*e* 1801, or — 1804, 1833.	*e* 1872, 2406.	*e* — 2445.

Plu. Same for all genders.

N. *e* 1865, 2031, 2250.

G. Lacking.

D. — *e* 1791.

A. *e* 1802, 2193, also (privei, 2188).

Comparatives.

2011 leng, vylloker (adv.), rather (adv.), ere (adv.), rather 2051 (adv.).
prestere 2106, heƍere, worse, next.
(moche-more) denoting intensity, 2034; denoting number *mo*, 2068, 2085, forthere.

Superlatives.

meste (the meste schrewen).

Numeral Adjectives.

O, on, 2253, enes (adv.); tuo 2277, tuei 2193, tueye 1914; threo, 1802, 1929; four 2001, foure 2405; vyf, 2068; six, 1850; sove, 2503; threttene, thridde, 1847, 2430; furthe, 2446; vyfte, 2178; sovethe, 1849.

ARTICLES.

The definite article is *the* for all genders and cases. The indefinite article is *a*, *an*, and *ane*.

ADVERBS.

Adverbs are formed from adjectives by adding *-e* or *-liche* (Ags. *lice*, modern English *-ly*). The former are the more frequent; as, sore, occurring 1787 and often elsewhere; faste, 1827, and frequently; wide, 1843; foule, 2070; softe, 2183.

Of the adverbs in *-liche*, a few are as follows: —

1897 myldeliche	1950 stilliche	1990 noleliche
2092 baldeliche	2205 duffulliche	

VERBS, WEAK AND STRONG.

Personal Endings.

STRONG.

Indicative.	*Present.*	*Subjunctive.*	*Imperative.*
SING. 1. — e	SING. 1.		SING.
2. — est, or st	2.		2. — —
3. — eth, th, — (2161)	3. — e		
PLU. — eth	PLU. — e		PLU. — eth

Indic.	*Past Subj.*	*Past Participle.*
SING. 1.	SING. 1.	
2. — e	2.	i —
3. — —	3. — e	
PLU. — e (1836), — en, i — e 2068 (2d pers. pron. omitted).	PLU. — e	

WEAK.

Indicative.	*Present.*	*Subj.*	*Imper.*	*Infin.*
SING. 1. — —	SING. 1.	SING. 2. —		— e
2. — est	2. — e, — i, —		— e	— i
3. — eth	3. — e	PLU. 3.	(assoili)	
PLU. — eth	PLU. — e		— i (granti)	(passi)

	Past.	*Past Participle.*
SING. 1.		
2.		
3. te, ede (1793).		i — ed
PLU. — eden (1803).		y — ed
— ede (1804).		

STRONG. — PRINCIPAL PARTS ARRANGED IN ALPHABETICAL ORDER.

Present.	*Past Sing.*	*Past Plu.*	*Past Part.*
abide (abyd, imper.)		abide	
afonge (fongen, inf.)			
agros			
aloute			
atake (bitake)	aftoc, bitoc (toke) 1832		
bede (inf.) (biddeth = beten	(bidde = bitten	bad (both meanings).	bede, beden, bitten
bere		bere	ibore ibounde
	bigan	gonne	
	bicom		
	biȝat (forȝet, underȝet)		
breken	brak	breke	
	cast(e)	caste	
clipeth			
		chose	
		crope	
comest	bicom	come (perhaps subjunctive) 1817, 1830, 1833.	icome
do, don, done	dude (1837)	duden	ido
drawe (drauȝ imper.)	drouȝ	drowe	to-drawe idryve
	et	ete	
falleth	bifal	falle	
fleo		flowe	
forgyve	ȝaf		
forȝet	forȝet, underȝet		forȝute
forsake	forsoc	forsoke	
fynde		fonde, funde	founde, fonden
geve (geveth, imper.)	ȝof		
go	ȝeode	ȝeoden	gon, agone (2332)
hongen			
holden (hold, imper.)	huld, huld(e)	bihulde	
helpe			
hoteth (1st plu.)	bihet		
lie (= lügen)			
ligge (inf.), lith	lay or lai	leye	
nyme (binyme)	nom	nome	binome, ynome
	let	forlete, lete	
	quath		

Present.	*Past Sing.*	*Past Plu.*	*Past Part.*
rede		radden	
ride	rod		
seoth	say (1966)		
iseo	iseȝ, iseye		isene
sitte	sat		
singeth, singe	song	songe	
schryve			ischryve
		slepe	
smiteth, smyte	smot	smyte, smyten, smite	ismyte
spekt		bispeke	
	stonk		
swerie (inf.), swere (subj.)		swor(e)	
totere			
sle(e) 1803, sle	sloȝ	slowe	aslawe
		schove	
stent (3d sing.) stonde (inf.), stonde (pl.)	under-, a- + stode	stode	stonde
			iwasche
	therste	therste	
uncover			
	wep		
		wode	
		woke	
			irite
1 worde, 2 worst, 3 plu. worþ(e), 3 worth (2034), as passive auxiliary.			
		ȝulde	

IRREGULAR WEAK VERBS. — PRINCIPAL PARTS.

Infin. Pres.	*Past.*	*Past Part.*
wende	wende	
	sende	
	ihurde	
telle	tolde, pl. tolden	
kepe	kipte	
		icouth
bringe	broȝte	ibroȝt
	roȝte	
lede	ladde	
Pencan	poȝte	
owe	auȝtest (2d sing.)	
1 sigge, 2 saiest (sai, imper.)	seyde, seide, sigge (subj.)	
	sede 2070	

ANOMALOUS, PRETERITE, PRESENT, ETC.

Wesan.

Indic.	*Present Subj.*	*Infin.*
SING. 1. am	SING. 1. beo	beo
2. ert	2.	
3. is	3.	
PLU. beoth	PLU.	

SING. 1. *Indic.*	*Past Subj.*	*Past Part.*
2. were (2066)	SING. 2. were 1862	beo
3. was, nas	3. were	ibeo
PLU. were, weren	PLU. were	

Habban.

Indic. *Pres.*	*Pres. Subj.*	*Imper.*
SING. 1. have	SING. 1.	PLU. habbe
2. havest, hast	2.	
3. nath, hath, habbeth (2044)	3. habbe	*Past Part.*
PLU. habbeth		habbe
PAST SING. 3. hadde	PLU. hadde	

Magan.

Indic.	*Pres.*	*Subj.*
SING. 1. may, mai	PAST SING. 1.	
2. miȝt (?)	2. miȝte	PAST SING. 3. miȝte
3. mai	3. miȝte	
PLU. mowe	PLU. miȝte	

Witan.

Indic. *Pres.*	*Subj.*	*Ind.* *Past.*
SING. 1. wot		SING. 1.
2. wost		2.
3.		3. nuste, wiste
PLU. wite		PLU. wiste

Don.

Indic. *Pres.*	*Past.*	*Subj.* *Pres.*
SING. 1. do	SING. 1.	SING. 1.
2.	2.	2. do
3.	3. dude	3.
PLU. doth	PLU. duden, dude	

Motan.

Mote is the only form that occurs, and it is found in first and third persons of the present subjunctive.

Must.

Moste is the only form; occurring in the plural of the past subjunctive.

Willan.

Indic.	*Present.*	*Subj.*
SING. 1.	nele, wole, neli	
2.	nelt, wilnest (2047 from wilnian)	
3.	wole	
PLU.	wolleth	

Indic.	*Past.*	*Subj.*
SING. 1.		SING. 1.
2.	woldest	2.
3.	wolde	3. nolde
PLU.	wolde, nolde	PLU. wolde

Shall.

Indic.	*Present.*	*Subj.*
SING. 1.	schal	
2.	schalt	
3.	schal	
PLU. 1–3.	{ schulleth / schulle } schullen	

Past Subj.

SING. 1.
2.
3. scholde, schulde

NOTE. — Attention is called to the fact that the Greek letter Zeta is substituted for the Old English letter, which corresponds to *s*, *y*, and *g*, at different times.

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WILLIAM PARSONS ATKINSON.

MEMORIAL REPORTED BY A SPECIAL COMMITTEE CONSISTING OF PROFESSORS JOHN D. RUNKLE, CHARLES R. CROSS, AND DAVIS R. DEWEY, AND UNANIMOUSLY ADOPTED BY THE FACULTY OF THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY, AT A MEETING HELD APRIL 16, 1890.

THE recent loss which the Institute has sustained in the death of the Emeritus Professor of English and History calls for more than ordinary consideration.

Professor Atkinson was the senior of our number in regard to age, and, with a single exception, also in the order of appointment. His labors with us began in 1865, and continued unbroken until the date of his resignation, less than a year since, at the close of a half-century of teaching. He was one of the first among professional teachers to appreciate the essential truth and large importance of the radical educational ideas underlying the scheme of instruction at the newly established School of Technology, a point especially worthy of note in view of the fact that his own training, his especial professional interests, and all his associations would naturally have led him to cling to the older system of a purely classical training. But he clearly saw the importance of the new methods of education of which the Institute was the leading exponent; and so was led, almost at the very outset, to ally himself to an institution unpopular among his associates, and unappreciated except by a very small circle of his friends. And as time went on, no one watched with more honest pride and generous satisfaction its slow but certain growth to its present position.

In the work of the Faculty, our late associate bore a large and important part, — how important can be realized fully only by those who have been conversant with the inner history of our various courses of

instruction for the past twenty years. During a large portion of this time he was not only the sole distinct representative in this body of the study of the humanities, but for years he was the only instructor of any grade in history, political science, literature, and philosophy.

The many hours consumed in the lecture-room, in which he met all classes of students, and the large amount of time spent in the preparation of his routine work, laid upon him a burden which only the strongest belief in the need of such training as he gave, and the most generous readiness to render to the institution with which he was connected the best that he had to bestow, could have given him strength to carry. And, moreover, as many of us can remember, in earlier days, when students were ill prepared, often immature, and frequently very narrow in their views, the work of the Professor of English was especially perplexing and laborious.

But however great the discouragements that arose, his zeal never flagged, and by his uniform independence, honesty, good temper, and self-sacrifice he gradually brought the peculiar work of his department to be recognized by students and teachers alike as essential to the completeness of our system of training.

Professor Atkinson recognized at the outset that the character of his instruction must be peculiar, — that he must strike out in new paths, if he was to interest and instruct youth who were so receptive, energetic, and insistent on receiving practical information as those with whom he had to deal. He saw, moreover, that he could claim only a small portion of time, and still less of sustained attention, from them, since they must of necessity be quite absorbed in their professional work. And for this reason, undoubtedly, he made his lectures to be broad sketches of literature and history, rather than minute discussions of technical points or elaborate statements of the results of special researches, and endeavored by their suggestiveness, first, to interest his pupils, and, secondly, to teach them how to read, think, and judge for themselves. This end he certainly accomplished. Even the dullest and most careless student was interested in what was laid before him, and to those more competent to appreciate the instruction offered Professor Atkinson's lectures were a veritable inspiration.

Such work as this, the conscientiously performed duties of a teacher in general branches, leaves little external and material evidence behind it to remind one of its extent and usefulness. No imposing laboratory or important professional course remains as a monument of Professor Atkinson's labors. From the very nature of the case that is impossible. His memorial must be sought in the influence which his own person-

ality and the ideas imparted by him have exerted in the formation of the mind and character of his pupils. And certainly there has been no member of the Faculty to whom our graduates and other past students have confessed their obligations more frequently, or with greater uniformity, than to him of whom we speak, and none for whom they have entertained a warmer personal regard. They have always remembered his sympathy, his warm interest in their welfare, his constant readiness to help them by kind counsel, — in short, his entire friendliness to them, whether earlier, as pupils, or later, when they had entered into professional life.

And these same qualities were always observed in his relations with his colleagues of the Faculty; cheerful, conscientious, pacific, holding before himself and all others the highest moral standards, ungrudging of his time and unsparing of his labor, he did with heartiness whatever his hand found to do, and seconded every effort whose end was to build up the Institute, whether it worked for or against the immediate interests of his department or himself.

The Faculty are saddened by his loss. They will remember his labors with grateful appreciation, and cherish his memory with affection and esteem.

SOME OF THE PRESENT ASPECTS OF EDUCATION.[1]

BY WILLIAM P. ATKINSON, LATE PROFESSOR OF ENGLISH AND HISTORY.

[In the twenty-five years of Professor Atkinson's faithful service at the Massachusetts Institute of Technology, fully three thousand young men and women must have come under his teaching, and it is not too much to say that in the life of every one of these his was a lasting influence.

Many of his students were, of necessity, restless under teaching not directly related to scientific pursuits; ill prepared in literary and historical work, eager to make every moment of their Institute course of direct and tangible use, some of them could not appreciate the value of his instruction; but of the power of his personality, of his strong and elevating influence, there was, even to the dullest of them, no shadow of question. The genial smile of welcome, the intuitive understanding of boyish doubts and difficulties, the wise and helpful counsel, were never wanting.

His outlook upon life was sound and manly; there was nothing mean, nothing selfish, nothing petty, about it. Fortunate is the young man who is brought, even for a moment, into a moral atmosphere so pure and bracing as his! Fortunate are the students of the Institute's first quarter-century to have breathed this air!

How many of the alumni to-day, as they grow older, thank Professor Atkinson for showing them the right use of books! How many more of them, unable to follow the lines he pointed out, are grateful for the memory of the gentle, fine, courageous life that closed too soon!

To these, "his boys," the following paper of Professor Atkinson's will be of especial interest; showing, as it does, his views upon the immense changes in the ideas and methods of teaching which have taken place within his lifetime, and in which the Institute of Technology has been so directly concerned. It is a wholly informal lecture, somewhat curtailed, delivered by him, in 1882, before the pupils of the State Normal School at Westfield, Massachusetts. He had no thought of its publication, and, in view of the lapse of years, he would undoubtedly have been unwilling to print it. It is so characteristic of him, however, and so full of interest, that, with the authority of his family, it has been decided to present it almost as it was written, and, necessarily, without the benefit of revision and correction by its lamented author.

It should be borne in mind, in reading the paper, that it was written eight years ago, and was addressed to a special audience of future teachers. — ED.]

WHEN, the other day, I had the pleasure of receiving the invitation to address you on this occasion, and sat down to bethink me on what subject I should speak, I made an unpleasant discovery. I found that

[1] An Address to the Graduating Class of the Westfield State Normal School, June 27, 1882.

it was a great while since I had spoken at Westfield, and that meantime — as indeed many other things continually remind me — I had been growing old. I discovered that, in fact, I had become a veteran, going to address an audience for whom I ought to have some words of *gray-haired* experience. Now this part of veteran is a part I do not very well know how to play. In spite of the irrefragable testimony of the family Bible, I cannot for the life of me feel old. It is one of the privileges we teachers enjoy, that, living and working as we do among the young, we need not grow old so fast as other people. Nevertheless, however young I may be in feeling, I cannot but remember that it is now more than forty years since I, a mere youngster who ought to have been at school, was tumbled out of college with what was ridiculously called a liberal education, — tumbled out into this hard world, with hardly a penny in my purse, to scramble for a living as best I might in the capacity of a teacher. A teacher, Heaven save the mark! Why, it was at an age when boys nowadays are just leaving school to enter college, and you may conceive what the stock in trade of wisdom must have been with which I first set up in the business of educating my generation.

It was the unpleasant recollection of how many years ago that was which came over me when I sat down to consider what I should say to you to-day. It was the feeling that, whether I had it or not, I *ought* at least to have gathered since that far-off day some wisdom from experience to bestow on my younger brethren and sisters in the profession, which determined me to try to give you some of my own views of the progress of education in my time. I shall not be afraid of appearing egotistical, for I feel nowhere more at home than when addressing an audience of teachers, and for many years I have had the great and genuine pleasure of being a frequent lecturer in normal schools.

It was with nothing less, therefore, than a strong feeling of personal interest that I accepted the invitation to say a few words to you to-day, and determined to speak of some of the changes in the educational world which have taken place since I, a very green youth, first became a teacher. Of what may be called the petty politics of the educational world I have nothing to say, for I have long been out of them, though in past times I have served my turn on school committees and in the editor's chair. It is of the great secular change that has been slowly going on within my time in the whole philosophy of education that I would rather speak, — a change which, whether we are conscious of it or not, has been slowly

shifting the very foundations of the educational structure, and which can be compared in its importance only with that other great change of educational base which came about in the fifteenth century as the result of what we call the Revival of Learning. I shall begin by inquiring into the nature and extent of this change, whose progress has been about coeval with my experience as a teacher, for I have, as becomes a veteran, some views of my own about it, — views which, as perhaps also becomes a veteran, become somewhat more conservative, rather than more radical, as I grow older.

Of course, in speaking of the great change which has come over the methods and the philosophy of education, I refer first and foremost, though by no means altogether, to the potent influence of that vast new factor, modern physical science. Every great period in educational history is characterized by the predominance of some study, or some group of studies, as its chief instrument of culture. During the Middle Ages it was the scholastic philosophy. When the great Reformation of religion had forever broken the power of the mediæval Church, of which the scholastic philosophy was the educational instrument, the revival of learning brought to the front those newly discovered treasures, the languages, the literatures, and the art of ancient Greece and Rome; and what potent civilizers these were, what an incalculable debt the world owes them, a debt of mental training and mental discipline, a debt of refinement and elevation and enlargement of mind, is obvious to the most superficial student of history. But no one who watches the signs of the times can fail to see that the period of the predominance of classical studies has fully come to an end, and that we are fast entering upon an entirely new phase of educational development. Let us inquire what connection it has with our own educational history.

When our fathers emigrated to this country and laid the first humble foundations of our higher institutions of learning, they were concerned to provide first and foremost for the deepest of all human interests, — for that interest a care for which had led them to brave the perils of the wilderness, — the interest of Religion, in that form which they conceived to be the only true one. This once provided for, their only conception of the higher education was, as was inevitable, the conception they had brought with them from the Old World that a liberal education, so far as it was not religious, could be based only on the study of the Greek and Roman classics. Just as their religious creed was the legacy of the great Reformation, so their conception of the higher education was the legacy of that revival

of learning which was coeval with it. This higher education, too, was in their old home the monopoly of privileged classes: first, of the clergy, the only educated class of the Middle Ages; next, of the lawyers; and finally, when medicine had outgrown the control of barber surgeons and old women, of the doctors. Along with these in the home from which our fathers came were to be reckoned, by virtue of their feudal rank, the titled aristocracy who in their youth resided and were supposed to study at Cambridge and Oxford.

At first it was hidden from their eyes that this fifteenth century scheme was to prove altogether inadequate and unsuitable to the wants of the giant young republic they were unconsciously founding. In entire good faith they set to work to lay the foundations of their future Oxfords and Cambridges on the good old pattern; in good faith they endowed, in the good old way, the future Etons and Harrows and Westminsters which were to supply them; let us be thankful to our fathers for their generous care for learning. New England owes them a debt for the founding, in their day of small things, of her colleges and academies, which can never be repaid. These are what more than all else have given to New England her intellectual supremacy. To expect their founders to anticipate all the educational wants of the coming generations would be to expect a miracle. And yet they did partially anticipate those wants; for side by side with this old-world provision for the higher learning they planted the free common school for the education of the people. True, they were not free from that class bias which distinguished between the aristocratic and the plebeian in education. It was not till long after that a younger than any of the New England States ventured boldly to incorporate her University into her public school system. The aristocratic distinction of caste long hung about New England. The attempt is still made to galvanize her old academies into new life, and to make them the exclusive feeders of her colleges, — her common high schools are still looked upon in some quarters as belonging to an inferior order. I do not think that any one nowadays goes quite so far as that sturdy old Tory of a hundred years ago, Dr. Johnson, and is of opinion that it is not right, as he says, to teach reading beyond a certain extent in society. A hundred years has carried us beyond that, but plenty of Tories may still be found who doubt whether it is quite right to incorporate the higher education into our public school system, — whether the common people in their common schools have properly anything to do with culture.

On the other hand, a new principle has been steadily gaining ground among us, and that is, that in a republic like ours the higher and the highest education equally with the lower should be the care of the whole people and open to the whole people, — that this distinction of caste in education, this gulf between privileged and unprivileged, aristocratic and plebeian, is as much out of place in our educational as it is in our political arrangements, — that the higher education of a republic can no more be permanently managed rightly by sects or cliques or close corporations than can her politics, but is equally to be the care of the nation, — and, furthermore, that that higher education can no longer remain the monopoly of a few so-called learned professions. What marks the period of my experience in teaching seems to me to be just this struggle between the old and the new principles, the old principle steadily losing ground, the new principle steadily gaining. There has been, for instance, during that period a steady growth and progress in our public high schools, a steady decline in the relative importance of endowed academies. Colleges flourish in our land just in proportion as they partake of the new spirit of the times; and new instrumentalities, such as normal schools and technical and scientific schools and colleges, have come into being, which in my judgment have a great future before them.

In education we are everywhere encountering absolutely new wants, are face to face with absolutely new problems. How shall we meet them? By overturn? by revolution? by denying all the principles on which our fathers built, and ignoring all that we have inherited from them? Thus the mad French Revolutionists began with a new Year One, and set up a new worship of the Goddess of Reason. Thank Heaven, that is not the Anglo-Saxon way! The Anglo-Saxon way, when the inevitable progress of time brings some new element into the social problem, is to ask what is the minimum of change which it requires, — is to assume that we solved the problem correctly as far as our light went. If now a new factor appears, let us thankfully accept the correction it requires, thankfully adopt this new approximation toward an accuracy that can never be completely attained, but let us not in our haste say that wisdom was born with us, and that all that went before us is error.

It is this position that I have maintained from the outset in regard to that controversy of ancient classics *versus* science, a controversy which really lies very near the heart of all our educational discussions. If in mentioning this controversy I seem to you to bring up a threadbare subject, and one on which I have uttered myself by voice and in

print many times before, it is because I think it is a vital question, — a question that must come up again and again till we all reach a final agreement. Let me try, in a few words, to give you my idea of the point at which the discussion stands at the present moment.

The so-called classical and scientific question is only the name of one branch of a much deeper controversy which covers the whole ground of life and education, — it is but one phase of the struggle between Humanists and Realists which began in the fifteenth century. There are two, and only two, great divisions of studies, in one or the other of which all branches of human knowledge may be included, — the study of mind and the study of matter. How vain it is to attempt to form a complete education out of the exclusive study of either of these can best be learned by observing the effects of such exclusiveness on those who have tried the experiment. But the peculiarity of the educational situation of the day in which we are living is this, — that almost within the recollection of a teacher no older than myself a vast new body of physical knowledge has been poured out upon the world, which is not only of the highest importance to man's material well-being, but which furnishes a vast new field for the exercise of his highest intellectual faculties. Is it possible that an educational system organized before this body of knowledge came into existence can tranquilly move on in its old course precisely as though this new knowledge were non-existent? Let the lamentable history of some of our older institutions of learning, and the ill success of some of the new which have striven to ignore the situation, answer. Not to refer to our own experience, I lately had occasion to examine, as far as limited evidence enabled me to do so, into the condition at the present moment of those nine or ten great, old endowed schools of England which are usually, but very falsely, supposed still to represent her school education. In every instance, I found that those schools in the twenty years since I had once before attempted a similar examination through the printed evidence of a Parliamentary inquiry had flourished just in proportion as they had introduced the modern element into their teaching, while the one which had most successfully resisted it, old Westminster, was virtually in a moribund condition. I found, further, that in England new schools were everywhere growing into vigorous life without the prestige of the ancient name and the power of the accumulated wealth of Eton and Harrow. I was even led to doubt whether the great Victoria University, in the heart of busy, manufacturing, middle-class Manchester, was not rapidly becoming a far truer representative of the higher education of England than Oxford and Cambridge, with their crumbling towers and their picturesque antiquity.

But into this question of organization I will not now enter, for I want to ask a question that lies behind it. What part is this great, new factor of modern physical science to play in the education of the future? The day of the exclusive predominance of the ancient classics in our higher education is obviously at an end. Are classical studies to be abolished in favor of this gigantic intruder, or can some *modus vivendi* be established whereby the claims of both may be acknowledged? The first and crudest effort of this kind has been made in the attempt to add physical science in all its length and breadth to the old curriculum of higher study without any modification of the old curriculum itself. If the scheme of entrance examinations now in force in some of our older colleges be examined, it will be found that, while the old drill in the classical grammars is imposed on the preparatory schools and their teachers with even more than its ancient rigor, the demand is made for a show at least of elementary scientific knowledge, while the pressure of the spirit of the times has, in another direction, been so overwhelming, that the candidate once within the college doors is permitted to throw classical studies to the winds, and is given unlimited freedom of choice among all possible subjects of human inquiry. A more unlucky system, it seems to me, than this forced combination of all that is worst in the old methods with all that is most crude and superficial in the new, can hardly be imagined. It is one of those illogical compromises which mark a transition period, when the adherents of antiquated methods are slowly and unwillingly yielding to the pressure of the times, and all the more unwillingly because the adherents of the new ideas are so apt to be extravagant and unreasonable in their aggressiveness.

And extravagant and unreasonable in their aggressiveness, I myself think the adherents of modern scientific studies are often apt to be, raising thereby a needless prejudice against a cause which, taken on its true merits, has the justest and most reasonable claims on the attention of every teacher, and on no class of teachers a stronger one than on the teachers of our public schools. For this question of physical science *versus* classical studies is no question for colleges and academies merely, — it is a question which touches the best interests of all education, from the highest to the lowest. Unless we are prepared to accept the old-world distinction of aristocrat and plebeian in education, we have got to look it in the face as a part of the problem of national education, — a problem in whose right solution the humblest teacher in a village primary school has a direct and personal interest. For if I have one educational belief on which I feel more certain than on any

other, it is that sooner or later, and however far off the day may be, the education of this country will be all of one piece, constituted from top to bottom on one system, and that the only system worthy of a self-governing people, the broadest and most republican. However temporarily useful the relics of mediævalism may be, American education can never be permanently organized on mediæval, eleemosynary foundations.

Overlooking, then, the temporary confusion which reigns in our courses of study, the temporary alienation from popular sympathy of some of those higher institutions of learning which ought to be pioneers and guides in every new departure, I wish to ask what significance this question of the true place of physical science has for our public free schools; — a very momentous question, because, in my judgment, our public free schools are the foundation on which the whole fabric of American education is ultimately to rest. Now I said a few moments ago that I was myself a conservative. A teacher now for seventeen years in a scientific college, but not myself either a student or a teacher of physical science, — brought up myself by the most rigorous classical method, and for a good many years of my life a teacher of the classics, — I have had perhaps exceptionally good opportunities for viewing the question impartially from both sides, and I have come to two very definite conclusions as the result of this experience. I am persuaded, first, that the influence of modern science is bound within not a long period of time to recast and reorganize the whole method and spirit of our public school education, from the infant school upwards to the high school and the college; and, secondly, I am not less strongly persuaded that scientific studies alone can never constitute a true or a perfect education. Let me say a word on each of these points.

First, I say that the influence of the modern scientific spirit is bound within not a long period of time to reorganize the whole method of our public school education. And this I believe for two reasons, and I will give you the less important first. The less important ground for the immediate recognition of physical science as a far more important factor in our system of public school study than it is at present, or than it has heretofore been, is its overwhelming practical importance to the material interests of this great and growing country. Why, our public schools are not doing a tithe of what they ought to do in helping to develop the material interests of the country. Those material interests have grown into such vast proportions, — manufacturing, mining, commercial enterprises are carried on upon so vast a scale, — our

railways and telegraphs, our great Western farms, our mines of minerals and of coal, our great and ever-growing manufacturing interests, — I would add the interests of our maritime commerce, if a stupid and false economic system, a monument of our popular economic ignorance, had not destroyed it, — I say all, or almost all, these great interests are expanding into such gigantic proportions that it becomes a question of the greatest importance what our public schools are doing for their furtherance. It is too plain for argument that the well-being of the country requires its scientific education to keep pace with this gigantic development of its physical resources.

Now for seventeen years I have been one of the examiners of candidates for admission to the scientific college in which I teach. I have only just come from sharing in the examination of one hundred and fifty such candidates of the average age, as I found, of a little more than eighteen years. And these candidates have come from high schools and academies in all parts of the country. The papers we have been examining are set to test the qualifications of these eighteen-year old candidates for entering on a course of scientific study as a preparation for taking responsible positions in practical life, such as require a preparation of scientific knowledge, — as chemists, as architects, as mining, mechanical, and civil engineers, as naturalists, and last, but not least, as practical men of business to superintend and guide manufacturing and commercial enterprises. What is the story that these papers teach us? Is it that our public school system, from the primary up to the high school, is thoroughly organized as it should be to meet this great practical want of the nation? Do our examination papers show us that there is a practical vitality in the elementary schooling of our boys and girls at all corresponding to the immense vitality of the practical life of the country into which they are so soon to enter? I wish I could answer in the affirmative. Nothing is further from my purpose than to underrate the value of our public schools even as they are, and I am not unconscious of the progress they are constantly making. How can their importance be overrated! Believe me, they are the sheet-anchor of this republic. If we lose them, if we neglect them, if we are unfaithful to them, we shall forfeit our free institutions. I think nothing less than this can be said of our public schools.

And for this very reason I cannot look without alarm at the way in which just now they lag behind the demands that may justly be made on them. No, our examination papers do not tell the story they ought to tell of practical efficiency in our public school system

on the side of elementary science teaching. We cannot get what we have a right to demand on the part of young men and young women of eighteen from our public high schools, much less from our classical academies. More than by all other causes we are crippled in our own work by this want of preparation. We are forced to do the work of the schools, and begin with giving a belated instruction in the first principles of science. Many, I might say most of our students, have to begin with us at the very foundations of science, by training their eyes to see and their hands to manipulate in our laboratories, and by laying over again more firmly that foundation of mathematical knowledge without which no firm scientific superstructure can be built. And this can only be done, without overtaxing them, by sacrificing to a great extent those general studies which at the age of eighteen they ought to be pursuing, and might pursue if only their scientific preparation had been better. I speak from much experience here, for the sacrifice has to be made in the direction of the studies which I teach.

What should a youth well trained in our public schools know at the age of seventeen or eighteen, if our public schools were all they ought to be? I say, that the work of scientific teaching which we are to carry on ought to be begun in the *primary school*, not by us. I say deliberately the primary school, and I add that the grammar school should continue it. It is as absurd that the high school should have to begin the work of science teaching as that we should have to begin it, as practically with many of our pupils we really have to do. For what after all is elementary science teaching? Is it the cramming of hard Latin and Greek technical words? Is it teaching children something about a lot of complicated and costly apparatus, electrical machines, and galvanic batteries, and strange chemicals that make explosions and bad smells? Such is an ignorant man's view of elementary science teaching, and he holds up his hands in amazement at the thought of subjecting the minds of children to such a strain, as he calls it, and gravely talks of the excellence of the good old Three R's, and buttons up his pockets tight when he thinks of the batteries and the chemical smells. The schools cost too much now, he says, — he 'll pay no bills for scientific gimcracks. And if the ignorant tax-payer were right, I should entirely agree with him; but in elementary science teaching such as I mean, the museum may be collected in the fields and by the wayside, the supplies can be bought of the village apothecary and the village grocer, and much of the apparatus can be whittled out with a jack-knife. What the ignorant tax-payer just now needs to learn is, that the most expen-

sive of all schools are just the schools which teach nothing but the Three R's; that the Three R's must indeed be taught, and taught thoroughly, but in order that they may be taught thoroughly something else must be taught with them; and that that something else is neither a smattering of Greek and Latin grammar on the one hand, nor a smattering of knowledge of costly scientific gimcracks on the other, but some adequate and just rudimentary conceptions of this wonderful world we live in. How to get that sort of knowledge into the child's mind, how to get that sort of teaching into primary and grammar schools, seems to me to be the most pressing educational problem of the day.

Now I have put this question of elementary science teaching upon the low utilitarian ground of its mere practical importance, of its money value, but the question can be argued upon much higher ground than that. Elementary science teaching has got to be introduced into our primary and grammar schools because physical science is an indispensable instrument for the development of the youthful mind, and therefore a true educational philosophy puts it there. Nothing is more absurd than the stupid and ignorant talk of stupid and ignorant people about the supreme excellence of the Three R's as instruments for the development of the youthful mind. The schools that confine themselves to the teaching of the Three R's, — and I will add to them that study which we are pleased to call English Grammar, — I say, the elementary schools which confine themselves to these studies on the plea of simplicity and thoroughness are really doomed to a career of impotence and barrenness, for their theory is directly contrary to the first principles of a sound educational philosophy. What is the divinely appointed instrument for the development of the powers of the youthful mind? Leave the child at liberty to answer the question as his nature prompts him, and you will soon know. You will find that Nature herself has appointed that it shall be the wonder and the mystery and the beauty of this wonderful and beautiful world in which we find ourselves, — a wonder and a mystery and a beauty that are stimulating his intellect and his feelings through every moment of the child's waking life, — unless by our false education we succeed in dulling all his natural sensibilities and destroying his natural curiosity. Is it a very hopeful way of helping in this divine education to shut Nature out of the schoolroom, and imprison the poor child in four bare walls with nothing but a spelling-book, a slate, and a grammar? But this is the good old method of the Three R's which has come down to us from days when science was unknown, and when books were held in such awe that a

knowledge of the art of reading was able to save a man's neck from the gallows. I am afraid that surer justice would be done nowadays by pardoning those who did *not* know their letters.

I say it is this superstition that education consists solely in book learning, and that schools have solely to do with books, that more than all things else hinders our schools from attaining their full measure of efficiency. We must emancipate ourselves from pedantry, we must learn that, though it is a good thing to teach children to read, it is a far higher thing to teach them to *think*, and that the two do not always go together. So far from that, the school organization may be so perversely and ingeniously wrong, that the reading of books may become a barrier in the way of thinking; so that the more the child studies books in school, the less he will know to any good purpose, the less and not the better prepared he will be for the real work of life. It is a sad thing to say, but I think this condemnation must be passed on much that we now call school education, — that it is a thing to save a child's mind from, if he is ever to be good for anything afterwards.

To my mind the situation seems to be this. A great and precious body of new knowledge respecting the laws that govern the material universe has been added to the world's intellectual treasures almost within the generation to which we belong; the intellectual current of the age runs strongly in the direction of scientific investigation; science imposes its methods on all the domains of thought; it is the educating instrument of our time, even as the new-found classics of Greece and Rome were the educating instrument of the times of the Revival of Learning. And it is met in some quarters much in the same way as they were. You have heard of the venerable, conservative ecclesiastic of the fifteenth century who preached to his flock in this way: "There is a new language, my children, called Greek, which some pestilent fellows are recommending to us. I bid you beware of it. It is the language of the devil, and whoever meddles with it is likely to go straight to hell." Such would seem to be the view of the venerable conservatives of our day respecting modern science. It is irreligious, it is atheistic, it unsettles the foundations of faith, — let us keep to the good old ways and go on mumbling our grammars. But the world never stands still. Each age has its own educating instrument, and the chief instrument of our day is that great body of new scientific knowledge which is its own peculiar product.

I have thus tried to do justice to the scientific movement of our day as a factor in education. Let us now turn for a few moments to the other side. Is there not danger as well as good in that movement?

We need have no sort of fear, it may well be said, for the future of scientific education in America. The needs of this new country for that kind of knowledge to help in developing its vast material resources are so great and pressing that its future is secure. Does not the danger lie in the other direction, — that literary studies will be carried away by this rising tide of science, and so our education will get too strong a materialistic bias? I will not deny that I think there is such a danger, and I will introduce what I have to say on this point by giving myself the pleasure of quoting a characteristically eloquent, though characteristically extravagant, passage from Mr. Ruskin.

"The great mechanical impulses of the age," he says, "of which most of us are so proud, are a mere passing fever, half speculative, half childish. People will discover that royal roads to anything can no more be laid in iron than they can in dust; that there are in fact no royal roads to anywhere worth going to; that, if there were, it would that instant cease to be worth going to, — I mean so far as things to be obtained are in any way estimable in terms of *price*. For there are two classes of precious things in the world, — those that God gives us for nothing, sun, air, life (both mortal and immortal), — and secondarily precious things which he gives us for a price; these secondarily precious things, worldly wine and milk, can only be bought for definite money; they can never be cheapened; no cheating nor bargaining will ever get a single thing out of nature's 'establishment' at half-price. Do we want to be strong? we must work. To be hungry? we must starve. To be happy? we must be kind. To be wise? we must look and think. No changing of place at a hundred miles an hour, no making of stuffs a thousand yards a minute, will make us one whit stronger, happier, or wiser. There was always in the world more than men could see, walked they ever so slowly; they will see it no better for going fast. And they will at last, and soon too, find out that their grand inventions for conquering (as they think) space and time do in reality conquer nothing; for space and time are in their own essence unconquerable, and besides did not want any sort of conquering; they wanted *using*. A fool always wants to shorten space and time; a wise man wants to lengthen both. A fool wants to kill space and kill time; a wise man, first to gain them and then to animate them. Your railroad, when you come to understand it, is only a device for making the world smaller; and as for being able to talk from place to place, that is indeed well and convenient; but suppose you have originally nothing to say. We shall be obliged at last to confess what we should long ago have known, that the really precious things are thought and sight, not pace. It does

a bullet no good to go fast, and a man, if he be truly a man, no harm to go slow; for his glory is not at all in going, but in being. . . .

"Gradually thinking on from point to point," he continues, "we shall come to perceive that all true happiness and nobleness are near us and yet neglected by us, and that till we have learned how to be happy and noble we have not much to tell even to red Indians. The delights of horse-racing and hunting, of assemblies in the night instead of the day, of costly and wearisome music, of costly and burdensome dress, of chagrined contention for place or power or wealth or the eyes of the multitude, and all the endless occupation without purpose and idleness without rest of our vulgar world, are not, it seems to me, enjoyments we need be ambitious to communicate. And all real and wholesome enjoyments possible to man have been just as possible to him since he was made of the earth as they are now; and they are possible to him chiefly in peace. To watch the corn grow and the blossoms set, to draw hard breath over ploughshare or spade, to read, to think, to love, to hope, to pray, — these are the things that make men happy; they have always had the power of doing these, they never will have power to do more. The world's prosperity or adversity depends upon our knowing and teaching these few things, but upon iron or glass or electricity or steam in no wise."

There is a half-truth in Mr. Ruskin's eloquent tirade, which in these materialistic days we must not lose sight of, — one of those half-truths in which Mr. Ruskin is so fond of dealing. It is wholly unjust to modern science to imply that its chief result is the growth of senseless luxury. The legitimate results of the growth of modern scientific study are two: first, that it affords a vast new field for the training of the human intellect; and, next, that it is so cheapening the production of material things, that it is slowly raising the mass of men out of their bondage to mere bodily toil, and giving them the opportunity to lead the lives of intellectual and moral beings. The practical result of all our modern science is, after all, only to cheapen things; but this is a very great result, because, by throwing the burden of production more and more upon the great forces of nature, it elevates the quality of man's labor, and by rendering life more easy throws open to all men advantages and privileges which were once the monopoly of the few, and a workingman can command to-day an education and a life which once a prince might have envied. With all deference to Mr. Ruskin, it *is* an advantage to us to move fast, if only we can be taught to move to good purpose and in the right direction.

And to move to good purpose no wholesomer counsel can be given

than his, that we must learn to look beyond the mere material benefits which spring from modern scientific progress. We talk very glibly of the distinction between physics and metaphysics, but who shall draw the line? We can go but very little way in our study of physical science without encountering the profoundest metaphysical problems; on the other hand, our metaphysics divorced from physics become a vain beating of the air, a spinning of cobweb theories. And so it comes about, that the last men to underrate the value of the moral sciences are the really great students of physical science, the last men to claim the domain of education as exclusively their own. The true spirit of modern science is a reverent spirit, recognizing, even when it penetrates farthest beyond the old boundaries of knowledge, the vast and infinite realm beyond, which no finite mind can ever comprehend; the spirit which moved Newton to say that he seemed to himself but as a little child picking up shells and pebbles by the shore of the vast ocean of truth, the spirit which moved Faraday to humble and reverent worship, the spirit which made Darwin as cautious and truthful as he was profound. We can never end with the study of matter; to the study of matter we must add its complement, the study of man, the study of mind. What shall I say of this other half of the domain of thought, — the domain of what I have called historical or moral studies? The term *literature* is so vague and misleading that I do not like to use it.

Here too I have to note a great deficiency, a sad lagging behind on the part of our school system, a failure to meet the needs and the demands of the spirit of the age. We have not yet emancipated our schools from mediævalism, made our school studies the true representatives of the atmosphere of thought in which we live. In saying this, I blame no one. So far from that, it seems to me the progress we are making is as great as could reasonably be expected. The school system of a free country can only represent the average intelligence of the country. Its aim is not to raise up a literary caste, separated, as in Germany, by a great gulf from the unintellectual life of the people that surrounds it. It is far better that a nation should educate itself, even if it educates itself slowly, than that its education, whether higher or lower, should be taken in hand by a paternal government, or by a caste of book men and *literateurs*. The education of a nation is a task far too great for mere bookmen. I look with most hope to those schools and institutions of learning which share most fully in the spirit of the nation, — which have grown most directly out of its wants, and are in closest sympathy with the social organism

of which they form a part. Their progress may be slow, their work may be modest work, but every particle of it tells.

Now, when we survey the domain of what I have called roughly historical or moral studies, — by which I mean the whole of that group relating to man, his thoughts, his feelings and actions, as distinct from the study of the laws governing this material universe which forms the stage on which his mortal life-drama is enacted, — I am forced to note the defect that here too our schools are not yet emancipated from that spirit of bookishness, that spirit of pedantry, which has come down to us from the times when an impassable gulf lay between the unlearned and the book-learned. Whether we choose to recognize it or not, that gulf has disappeared. There are no longer any privileged literary classes; the ancient badges which distinguished them, the masterships and the doctorates, have become almost unmeaning; the work of which they used to have the monopoly is now oftener done outside than inside their sacred walls. The most learned history of ancient Greece written in England in our day came, not from Oxford, but from the library of a busy London banker and member of Parliament, who had never been to Oxford or Cambridge, and who, if he had gone to either of those venerable seats of learning, would probably never have written his History. We have got to recognize the fact that a new era has dawned in the study of literature and history, as well as in the study of science, and that we teachers of literature must take a leaf out of the books of our scientific brethren, and learn to apply their methods to our own pursuits. Are we doing so, and by new methods putting new vitality into moral and historical studies? Steadily and slowly I think we are, but I am afraid only slowly. During the many years in which I have had the examining of the English entrance examination papers at our school, I note a gradual improvement in quality, a general rise in the intellectual level of the classes I have to meet. But I am almost ashamed of the papers I have to set. I cannot assume anything like that average level of literary culture which I think young men and young women of eighteen ought to attain at our high schools; much less can I assume that anything like system or logical method prevails in literary teaching. Literary instruction at present is the very realm of chaos and confusion, till it can hardly be said that any two of my students have been trained alike. I have pupils who have wasted their school time in an abortive attempt to learn Latin and Greek. I have students of the type of the thoughtful son of a country doctor, who told me that as a boy he had read and pondered the miscellaneous contents of his

father's library in the primitive retirement of his father's hay-mow, and who proceeded to show in everything he did that he had pondered them to good purpose, — that hay-mow having proved to him a true academy. I have, on the other hand, students of the type of the youth who returned to me a long list of English classic writers whose works he had been put through at a high-pressure school, naively adding, that, if there were any studies he *hated*, they were rhetoric and literature, and who forthwith proceeded to illustrate the beauties of cramming by showing that he knew nothing about his formidable list of authors.

However it may have been in the past, it can no longer be maintained that the study of the English and other living languages cannot be turned into an instrument of the strictest mental discipline. I look upon the chaos and confusion that reigns in this most important department of school work as the direct result of the influence of college classical examinations. Let these be changed by a frank acceptance of an equivalent for Greek, instead of the futile attempt to thrust all other studies into the unhappy schoolboy's course alongside of Greek, and a course of English study worthy of our high schools would instantly organize itself. Latin would maintain its place, but with a new character and added weight; the study of the living cultivated languages of the world would gain new dignity and importance as disciplinary studies; but, above all, the systematic study of the mother tongue, and all the wealth of its rich literature, would take its true position. Then the study would be organized on true principles, and the breath of life would be breathed into the dreary pedantry of school grammars. Our eyes would be opened to the truth that we are born into the possession of a language and a literature second to no other in power and beauty, — a language that more than any other seems destined to be the general instrument of communication among civilized men. One would think that its loving study ought to be found at the very heart of our school system, that it ought to be the foremost object of our educational care; but it is not that now.

Then, too, I think there will be a real chance for that group of studies which, for want of a better name, I must call historical or moral, and for which the study of language is the preparation. I do not much care to describe the condition of historical study in our schools as it is revealed to me by my examination papers. I am almost tempted to imitate that famous short chapter in Horrebow on the snakes in Iceland, and say there is *no* study of history. But then to justify such an assertion I should have to explain what I mean, for we all know that

compendiums of history are studied, and dates are learned by heart, and histories are sometimes read in our schools. Nevertheless, I think that the organization of historical studies into an instrument of real mental discipline is an important piece of educational work still remaining to be done. I wish time allowed me to state what seem to me the main factors in the problem; how it would involve the study and understanding not more of the events of remote nations and bygone times than of the events of the very day that is passing over us, and of the great moral and intellectual forces in the midst of which we live; how its foundation would be laid even in school by the study of those very forces. I am utopian enough to believe that the rudiments of governmental science, the outlines of the laws under which we live, the elements of that political economy in accordance with or in defiance of whose principles the great industries of this modern world are carried on, — I say I believe that the rudiments of these apparently formidable subjects should all be taught in school, in order that our boys and girls may grow up intelligent citizens, capable of understanding the story of the past and of taking an intelligent part in the doings of the present. That will be the true Civil Service Reform, and perhaps we shall not get it till then. But it is too late for me to attempt to work out this branch of the subject. It would require a lecture to itself.

I cannot close this very imperfect survey of the present aspects of education without noting one other great step in progress which is big with hope for the future, — I mean the admission of women to all the privileges of the higher education. When we consider how largely the education of the young, and especially the guidance of those first steps in intellectual progress which are of all others the most important, are, and always must be, in the hands of woman, there can be no more hopeful sign in regard to the education of the future than to see that woman's right equally with man's to the advantages of all the highest culture the community can give is fast being fully recognized. And it is not only in the direction of literature, but in the direction of science, that I see new hope here. Women are not only improving, they are diversifying their education. The world is discovering that there is as great a variety of talents and aptitudes lying hidden in feminine as in masculine brains. And though I am not one of those who are for confounding all distinctions of sex in education, yet I am sure of one thing, that we never shall know all that woman is capable of doing till we have given her the amplest opportunity to show what she can do. It is now some years since a young lady

wrote us from Vassar College, where she had just graduated, to say that the teaching of Maria Mitchell had interested her greatly in the study of physical science, and to ask the privilege of entering our chemical laboratory as a special student. The question of the admission of women to the privileges of our school was fought out over that young lady's application, and decided in her favor. And when the time came for the class to graduate to which she was attached, she modestly asked that she might be allowed to stand all the examinations and be considered a candidate for our scientific degree. She did stand all the examinations and received her degree, and she has ever since been the efficient head of a chemical laboratory for women, established and equipped in connection with our school by a society of Boston ladies organized for the purpose of promoting the higher education of their sex. Since then we have never been without young women in our chemical and physical courses. At our recent graduating exercises, not the least interesting or valuable paper was the account by a young lady of an original and elaborate series of chemical and microscopical investigations.

Young ladies and gentlemen who have invited me to address you, in those far-off days when like you I stood on the threshold of life, I called myself an educational reformer, and I have no doubt I was often a very rash and a very hasty one. What shall I call myself now that my hair has grown gray? Shall I renounce the title, and fall back into the ranks of the reactionaries? No: you see I call myself a reformer still, but years and experience have taught me how hard is the upward path of improvement, how slow and toilsome must be all true progress, — much labor with seeming little result, and yet honest labor never in vain. Above all, I have discovered that, if we are ambitious to be world reformers, there is but one place where we can begin the great work, and that is at home; that he who would improve others must be in constant process of re-forming himself. I would not discourage you from either task. To re-form ourselves is what we were set on earth for, and no one can honestly and patiently set about that work without bringing a blessed and wholesome influence on all who surround him. As teachers I cannot promise that you will not have many discouragements and difficulties to encounter, but there is no occupation in which you can engage where the rewards of faithful and disinterested service are higher, or where there are more incitements to a true and noble life. Will you allow me to give one piece of parting advice? Let me counsel you, as one who speaks from a suffering experience, to preserve your health, your bodily and your mental

health, — and you cannot have one without the other. The nervous wear and tear of teaching is very great; and better than much book-learning as a true capital for a teacher is the cheerfulness, the elasticity, the readiness of resource, which come from a sound body and a sane mind. Our profession suffers in no way so much as from the want of these. I cannot prescribe rules of hygiene for you. Let me only say that two great safeguards to health of body and health of mind are plain living and high thinking. And I should be untrue to my deepest convictions if I did not add, as greatest of all, that faith in God and that constant prayer for guidance without which, however little we may know about the method of its answer, I do not believe that any life can be completely sane or really happy.

THE DRY ASSAY OF TIN ORES.[1]

PART I.

BY HEINRICH O. HOFMAN, ASSISTANT PROFESSOR OF MINING AND METALLURGY.

THE tin deposits of the Black Hills, discovered some ten years since, are beginning to attract very general attention, and the question how to treat the ores to the best advantage has become a pressing one. No systematic investigation of this subject had been made until quite recently, when it was taken up by the Dakota School of Mines, and the ores tested as to their tenor and proper mode of treatment. In the assay laboratory numerous determinations were made of the percentage of black tin in the ore bodies, and of the metallic tin contained in it. The occurrence of tin in the Black Hills being different from that in other parts of the world, the usual mode of procedure had to be somewhat modified.

The object of this paper is to give the results of the writer's effort to discover the best method of assaying the tin ores in the dry way. His task divided itself under three heads, in the following order: —

1. To test the different methods of assaying purified black tin in the dry way.
2. To study the influence upon the assay of the different minerals associated with the black tin.
3. To find by what simple and rapid means the cassiterite could most effectually be freed from its gangue.

The apparatus used was that to be found in any ordinary assay laboratory (the pulp-balance being sensitive to 1 mgr.) as it was the aim to furnish quick and reliable methods with simple appliances. That dry assays of black tin have not the same degree of accuracy as wet assays is well known, but they suffice for the determinations necessary in metallurgical work. If great exactness is required, recourse must be had to one of the two wet methods, — that of Rose (fusing with sulphur and sodium carbonate, and converting the insoluble stannic oxide into soluble stannic sulphide), or that of Rivot (reducing the stannic oxide in a current of hydrogen to metallic tin, which is then readily soluble).

[1] Read at the Colorado Meeting of the American Institute of Mining Engineers, June, 1889.

The cassiterite occurring in the Black Hills tin ores is quite pure, as is shown by the following analyses[1] of lode tin and stream tin: —

Lode tin: —		
Occidental Mine	96.42	SnO_2
Tin Mountain	97.50	"
First Find	94.70	"
Stream tin: —		
Nigger Hill	92.60	"
Nigger Hill	93.00	"
Southern Hills	92.80	"

Stannite has lately been found in one locality. The cassiterite has an adamantine lustre, and its color varies from brown to black, the light varieties being rare. The minerals associated with the cassiterite that are of interest to the assayer are iron pyrite, arseno-pyrite, in a very few instances galena and blende, quartz, feldspar, mica, tourmaline, garnet, and columbite (tantalite). The sulphides occur rarely, and then always in small quantities; quartz and silicates form the gangue; columbite (tantalite), although sometimes found in the same veins, can always be separated by hand-sorting. When it has accumulated in the stream tin deposits, which occasionally happens, it remains with the purified black tin. Bismuth, copper, and tungsten, which occur frequently in the deposits of other countries, are found very sparingly.

I. The Assay in General.

Tin that is to be assayed in the dry way must be present in the form of oxide. The assay proper consists of applying in a crucible at a high temperature reducing and fluxing reagents, which bring the stannic oxide to the metallic state and convert the gangue into a liquid slag. The behavior of stannic oxide and of metallic tin at an elevated temperature causes, however, certain difficulties, which make the results often less accurate than could be desired. These difficulties are: —

First. That some of the resulting metallic tin is liable to be volatilized.

Second. That metallic tin decomposes the alkali carbonates used as fluxes, forming stannates, which enter the slag.

Third. That at the temperature required to reduce the stannic oxide, other metallic oxides become also reduced to the metallic state and alloy with the resulting tin button, and vitiate the assay.

[1] Transactions of the American Institute of Mining Engineers, Vol. XVII. pp. 595, 596.

Fourth. That sulphates are reduced, or, if sulphides are present, they combine directly with the metallic tin and carry it into the slag.

Fifth. That the gangue in the ore is always siliceous. This causes loss, as the stannic oxide, while passing through the intermediate state of stannous oxide, combines with silica and silicates.

Sixth. That stannic oxide is liable, before its reduction has begun, to combine with the basic fluxes used in assaying and form stannates. This liability increases as the quantity of the flux is increased.

Thus it will be seen that the results can be too high (as when iron enters the tin button) or too low (as when tin is carried off in the slag as stannate, sulphide, or silicate), and that all impurities should be removed, if possible, before the actual dry assay takes place. The making up of the charge must also be regulated to counteract as much as possible the bad influences of the basic fluxes.

The crucibles used in assaying tin ores are the ordinary clay crucibles, the plumbago crucible, and the porcelain crucible. The naked clay crucible has the disadvantage that the basic fluxes attack the clay and form silicates, which are liable to slag some of the tin. From the results obtained by the writer, it would seem that the material of the Battersea crucibles used by him must have been much better than that of the crucibles referred to by Berthier,[1] as no appreciable loss resulted from their use. P. Ricketts[2] recommends the lining of the crucible with chalk. This is probably to avoid the action of the basic fluxes on the acid material of the crucible, as the calcium oxide, being a strong base, protects the clay of the crucible from the corrosive action of the alkali, and thus hinders the formation of an alkali silicate. An outline of his method having been obtained from Dr. Ricketts, the lining was made from the so-called "prepared chalk" of the druggist in the following manner. The prepared chalk was mixed with water in an enamelled iron dish to the consistency of cream. It was then allowed to stand, that the coarse, gritty particles might settle and collect at the bottom. The chalk emulsion, now freed from impurities, was poured off and was ready for use. To line the crucible, it was half filled with the chalk mixture, the upper half being coated by giving the crucible a rotary motion, after which the excess of the liquid was poured off. The crucible was then placed bottom up for five minutes to drain, after which it stood for two hours to dry, and was then ready for use. The coating obtained was thin, uniform, and smooth. If the crucible be filled entirely with the mixture, and this be allowed to

[1] Traité des Essais par la Voie Sèche, Liége, 1847, Vol. II. p. 485.

[2] Notes on Assaying, 8th ed., New York, 1886, pp. 88, 89.

remain a little while before being poured off, the coating will be less uniform on the sides, part of the naked crucible will be exposed, and the lining at the bottom, being too thick, will crack when it dries. If a greater thickness is wanted than can be obtained by one coat, it can only be properly put on, a layer at a time, as above, until the desired thickness is reached. The effect of the lining was disappointing; it did not, as will appear later, prove useful, but rather the reverse.

Another highly recommended lining is one of charcoal. The fluxes do not act chemically upon it, and its reducing power makes it unnecessary to add any charcoal to the assay; a reducing atmosphere is kept up during the entire operation. Berthier[1] states that he obtained 4 per cent more tin in a brasqued crucible than in a naked one, although even then he recovered 2 per cent less tin than was present.

The following method of brasquing, which differs somewhat from any of those described and recommended in the books, proved the most satisfactory. Pine charcoal is pulverized and screened through a 40-mesh sieve; the fine powder is then mixed with molasses and water in a suitable flat dish, and kneaded with the fingers until a homogeneous mass is obtained, which, when squeezed in the hand, coheres into a lump, and has not sufficient moisture to adhere to the hand.

Different proportions of molasses — the ordinary New Orleans molasses being used — and water were tried. Molasses alone with charcoal gave a sticky, shiny mass, which cracked on the sides and bottom of the crucible as it dried; two parts by volume of molasses and one of water behaved better, only the bottom cracking; three parts of molasses and one of water gave the desired mixture.

The crucible is moistened by dipping into water and quickly removing it. It is then partly filled with the charcoal paste, and this is pressed with the fingers against the sides and the bottom, a slight excess of material being used. Then a conical wooden plug, which has been dipped in water, is forced by hand into the charcoal paste and turned round and round, the excess escaping over the edge of the crucible. A few light taps with a mallet help to make the lining more compact. The wooden plug has a mark, and when this reaches the upper rim of the crucible the plug is withdrawn with the same rotary motion with which it was forced in. It has the form of a long cone, the point being slightly flattened. Its size is such that when forced into the crucible down to the mark, the lining at the bottom will be 3/8 inch thick. This thickness decreases until near the upper edge of the crucible, where it is only 1/8 inch. It is then rounded off until even with the

[1] Traité des Essais, 1847, Vol. II. p. 485.

inner edge of the crucible, and the sides are polished with a strong test-tube to prevent any particles of tin from adhering, and to obstruct the filtering of fluxes through it. In drying, it is best to place the crucible upside down; otherwise the lining is liable to separate from the wall, especially if it dries too rapidly. When a little skill has been acquired, it takes four and a half hours, including the preparation of the mixture, to line fifty Battersea crucibles, half of size F and half of size C.

It will be seen that this lining is quickly made; it dries quickly without cracking, and is hard when dry. After being in the fire, it is readily removed from the crucible, which can be used again, and is so compact and hard that it is difficult to break the thin upper edge with the fingers, while the thick lower part has to be broken with the hammer.

In summing up the subject of brasqued crucibles, it may be said that, although charcoal linings are perfect from a chemical point of view, their use has practical objections because of the separating of fine tin from fine slag in the pan. The results to be described will show that crucible linings are, in fact, unnecessary.

The plumbago crucible stands between the naked and the brasqued crucibles, for while the action of the fluxes on the clay is weakened by the plumbago, the neutral character of the latter is slightly modified by the clay that binds it together. Plumbago crucibles are frequently used in the Cornish assay, but we shall see that this method is unreliable, and, as the plumbago crucible is expensive, more need not be said about it.

Finally, the porcelain crucible used by Levol has the same disadvantages as the naked clay crucible, although in a less marked degree.

II. Preparation of the Ore for the Experiments.

The more representative lode tin not being available in sufficient quantities to be made the basis of the following experiments, stream tin from the Nigger Hill district had to be resorted to. The cassiterite of the Bismarck district would have been the most desirable of all, as it is the purest. It is found in quartz, and has comparatively no deleterious minerals occurring with it. This makes the separation of black tin from the gangue simple and perfect. The other lode tins, as well as all the stream tins of the Black Hills, have a number of minerals occurring with them which are difficult of removal; for instance, garnets.

The first pulverizing of the ore was performed with the distance of ⅛ inch between the faces of the sample-grinder. The crushed ore was then screened through a 40-mesh sieve. At the second grinding the faces of the machine were brought somewhat nearer together, and the sifting afterwards repeated. The operation was continued until the ore had all become fine enough to pass through the sieve, with the exception of some mica, which was thrown out. This gradual method was used in order to prevent the cassiterite from becoming too fine, as a large amount might thus have been lost in the washing of the ore. The pulp obtained was then roasted in a muffle to decompose any sulphides and arsenides that might be present. After roasting, and while still hot, it was thrown into cold water, to make silicates, not otherwise attacked by acids, decomposable. It was also hoped that the cassiterite might thus be made more readily reducible, an expectation which the event fulfilled. The ore was now boiled with nitro-hydrochloric acid, to remove all soluble metallic compounds; this was continued until no more iron was perceived to go into solution. The ore was then washed with hot water and transferred to the gold pan, where the

TABLE I.

Passed through sieve No.	Remained on sieve No.	Grams.	Per cent.
40	60	90	18
60	80	80	16
80	100	65	13
100	..	265	53
..	..	500	100

panning was carried on irrespective of the loss of some fine tin which floated off with the tailings, giving them a chocolate-brown color. As the main object was to obtain a pure black tin, the loss was unavoidable. The panning was continued until no more impurities were visible to the naked eye; under the microscope, however, particles of garnet with a little quartz were still visible. The black tin obtained was dried and intimately mixed by passing it repeatedly through a 20-mesh screen and rolling it on glazed paper. The now uniform ore was sampled down, and the final average sample pulverized in an agate

mortar and put aside for chemical analysis. The weight of the purified black tin obtained, which was to form the basis of the experiments, was 7,968 grams. To find the size of the different particles, 500 grams were taken and screened through different-sized sieves. The result of the mechanical analysis is given in Table I.

The very brittle character of the cassiterite will be seen from this table, as 53 per cent of the entire ore passed through a sieve of 100 meshes to the linear inch, although, as has been said, the greatest care was taken to grind in such a manner as to produce as small an amount of slimes as possible.

III. Analysis of the Purified Black Tin.

In making the analysis of the average sample of the purified black tin, the Rose method was followed, as modified by Chauvenet, who substitutes potassium carbonate for the sodium carbonate commonly used. Two separate determinations were made, taking 0.5 gram in each case with 3 grams of a mixture consisting of equal parts of potassium carbonate and sulphur, and fusing in a No. 1 Royal Berlin porcelain crucible. The first sample was heated three quarters of an hour over a Berzelius alcohol lamp, and then for the same length of time over a gasolene lamp, after which the fused mass was treated with water in a beaker. Brown, gritty cassiterite now became visible at the bottom, showing that the decomposition was incomplete. The soluble potassium sulphostannate was then filtered off, and the filtrate acidulated with sulphuric acid and put aside; the residue on the filter was treated with hot dilute nitric acid, and the iron, manganese, and lime solution also kept. The residue was again fused with potassium carbonate and sulphur, but in a different way. The No. 1 porcelain crucible was placed within a No. 3 porcelain crucible, and this within a No. 6 plumbago crucible, the bottom of which had been filled with refractory material, that the porcelain crucible might be nearly as high as the plumbago crucible. The three crucibles were then each covered with a lid, and the fusion performed in a pot-furnace, heated with anthracite coal at a moderate red heat. This lasted one hour. The fused mass was then treated as before, and the resulting decomposition found to be perfect. The fusion for the second analysis was made directly in the three crucibles, and was complete in one operation. To make sure of this, the fusion was repeated, and, on acidulating the aqueous solution with sulphuric acid, the precipitate was found to have the milky white color of sulphur.

The reason that the first fusion over the gasolene lamp was not successful, although prolonged until the fused mass became dry, must be sought in the fact that the air was imperfectly excluded. By fusing in a crucible which is enclosed in others, this exclusion can be entirely accomplished; in fact, upon removing the outer lid when the crucible had been taken from the fire, but had not yet cooled, a strong odor of sulphurous acid was perceived, showing that not only had the air been excluded, but that the fusion had taken place in an atmosphere of sulphur.[1]

The analyses were both made by the usual methods for separating iron, manganese, and lime, and the results corresponded so closely that they can be expressed by a common average: —

SnO_2	86.24 = 67.84 Sn
Fe_2O_3	2.62
Mn_2O_3	0.50
CaO	0.66
MgO	trace
Insoluble	9.39
	99.41

If this be compared with the analysis of the purest crystal of Nigger Hill stream tin[2] that could be found, —

SnO_2	93.06
Fe_2O_3	3.08
Insoluble	3.90
	100.04

it will be seen that the cleaning of the stream tin, as described above, was very successful, the ore sample showing only 2.4 times as much insoluble residue as the crystal.

IV. The Assay in Detail.

The different methods of assaying black tin in the dry way may be grouped under two heads: —

A. Those which aim at finding the actual amount of tin contained in the ore.

B. Those which determine approximately how much metallic tin can be recovered by treatment on a large scale.

[1] This use of the three crucibles was suggested by Dr. W. P. Headden, Professor of Chemistry at the Dakota School of Mines.

[2] Report of the Dakota School of Mines, 1888, p. 142.

The first class divides, according to the character of the resulting metal, into methods (*a*) yielding a button of metallic tin, and those (*b*) yielding an alloy of tin with copper or iron.

Class A (a).

1. *The German Method of Assay.* — This method, as given by Kerl [1] and by Balling,[2] is as follows:—

Five grams of ore are intimately mixed with 0.75 to 1 gram of charcoal dust, and charged into a clay crucible; on top are placed 12.5 to 15 grams of black flux (or the substitute, 2 parts of potassium-sodium-carbonate and 1 part of flour), with 1 to 1.25 grams of borax glass, then a salt cover, and, finally, a piece of charcoal. The crucible is covered, heated in a muffle or a pot-furnace at a moderate, gradually increasing temperature, until the boiling has ceased, and then for from half to three quarters of an hour at a white heat. The crucible is removed from the fire, broken when cool, and the tin button weighed.

The process that takes place is simple. With the gradually rising temperature the tin becomes reduced to the metallic state by the charcoal, with which it has been intimately mixed, while any ferric oxide contained in the cassiterite will be reduced only to ferrous oxide and taken up by the slag. At a certain stage the black flux, or the flour of the substitute, becomes decomposed, the result being that finely divided carbon is uniformly distributed through the flux. This hinders any particles of stannic oxide from combining with the alkali when fusion begins, and assists the reduction of particles of ore that have not been completely converted into metal by the charcoal. The active fluxes — potash, soda, borax — combine with the gangue contained in the ore and form a slag; the neutral salt cover assists in making the slag liquid, and thus favors the collecting of tin particles and prevents prills from adhering to the sides of the crucible; the charcoal finally, being slowly consumed, furnishes the reducing atmosphere above the charge. When the reactions have taken place and the fusion has become tranquil, the different parts settle according to their specific gravities: at the bottom, the tin button; next, the borax slag; above this the salt slag; any unconsumed charcoal remaining on top.

In carrying out the assays the above method was closely followed. The crucibles were size F, of the Battersea make; the charcoal was made to pass a 40-mesh sieve before it was mixed with the ore; the

[1] Metallurgische Probirkunst, Leipsic, 1882, p. 412.
[2] Die Probirkunde, Brunswick, 1879, p. 391.

black flux substitute — 2 parts of potassium carbonate and 1 part of flour, to which 1 gram of borax glass had been added — was used for flux.

The charge melted readily, and the slag was very thin, showing when cool a smooth surface. The crucible was not corroded by the fluxes, and no particles of tin were visible at the sides. On breaking it, the salt slag separated readily from the borax slag, the former being coarsely crystalline and brittle. It had a resinous lustre, was subtranslucent, and of a dark purple color. This color must be attributed to finely divided carbon resulting from the charcoal on top. On dissolving the slag in water, a fine black slime remained. This proved to be pure carbon, and the solution showed only a trace of iron. The borax slag was vitreous, hard, subtranslucent, and olive-green. The button separated well from the slag, was white, bright, easily cut, and malle-

TABLE II.

No of Assay.	Resulting Tin.							
	Total.		In Button.		In Scales.		In Siftings.	
	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.
1	3.379	67.58	3.37	67.40	....	....	....	....
2	3.379	67.58	3.37	67.40	....	....	....	....
3	3.379	67.58	3.37	67.40	....	....	....	....
Average	3.379	67.58	3.37	67.40	0.001	0.02	0.008	0.16
4	3.380	67.60	3.31	66.20	....	....	....	....
5	3.370	67.40	3.30	66.00	....	....	....	....
6	3.370	67.40	3.30	66.00	....	....	....	....
Average	3.373	67.46	3.303	66.06	0.015	0.30	0.055	1.10

able; scrapings were not attracted by the magnet, showing the absence[1] of iron. The tin, however, did not all collect in the button, as usually happens with tin assays. Prills remained suspended in the slag, and some adhered to that part of the crucible in contact with it.

[1] Berthier, Traité des Essais, 1847, Vol. II. p. 473.

In the assays, the salt slag, the borax slag, and the lower part of the crucible were ground together and screened through a 40-mesh sieve. The scales of each set of assays were weighed together, and the fine pulp of the same set panned, and the resulting fine tin weighed. It seemed advisable to do it in this manner, because the amount of scales and siftings in the separate assays was so small that it might easily have caused an error in the results.

If the percentage of tin recovered in the assays Nos. 1, 2, 3, 67.58 per cent, and in Nos. 4, 5, 6, 67.46 per cent, be compared with the actual amount of tin present, 67.84 per cent, it will be seen that the results obtained by this method are all that could be desired. The two averages differ only by 0.12 per cent, which is also very favorable. That assays Nos. 1, 2, 3 agree accurately is mere accident. It will be seen that 67.40 per cent collected in the button, and that only 0.28 per cent was scattered through the slags and adhered to the crucible. Assays Nos. 4, 5, 6 agree within 0.2 per cent. Less tin was collected in the button than in Nos. 1, 2, 3, and the total result is slightly lower, although still very good, i. e. accurate within 0.5 per cent.

TABLE III.

No. of Assay.	Resulting Tin.							
	Total.		In Button.		In Scales.		In Siftings.	
	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.
7	3.320	66.40	3.25	65.00	....	....	....	....
8	3.310	66.20	3.24	64.80	....	....	....	....
Average	3.315	66.30	3.245	64.90	0.015	0.30	0.055	1.10
9	3.000	60.00	2.93	58.60	....	....	....	....
10	2.850	57.00	2.78	55.60	....	....	....	....
Average	2.925	58.50	2.855	57.10	0.015	0.30	0.055	1.10

To see what influence would be produced by variations of time and temperature, assays Nos. 7 to 10 were made, increasing the time to two hours with Nos. 7, 8, 9, 10, and keeping Nos. 9 and 10 hotter than Nos. 7 and 8, and these last hotter than Nos. 1 to 6.

Table III. shows that the loss in tin increases if the crucibles are left

too long in a very hot fire; the buttons, however, remaining free from iron.

Although the results in the naked crucible were entirely satisfactory with normal conditions of temperature and time, the chalk-lined crucible was now used to see what effect would be produced upon the assay. Assays Nos. 11, 12, 13 were made in the same way as Nos. 1 to 6, keeping the temperature reasonably low. No difference could be perceived in examining the crucible, slags, and button, except that the upper part of the crucible was more tarnished than with the naked one.

TABLE IV.

No. of Assay.	Resulting Tin.							
	Total.		In Button.		In Scales.		In Siftings.	
	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.
11	3.393	67.86	3.38	67.60	....	....	....	....
12	3.373	67.46	3.36	67.20	....	....	....	....
13	3.363	67.26	3.35	67.00	....	....	....	....
Average	3.376	67.53	3.363	67.27	0.001	0.02	0.012	0.24

Table IV. shows an average result which does not differ much from that obtained in the naked crucible, but the assays vary as much as 0.6 per cent.

If time and temperature are increased, the difference from the results with the naked crucible becomes much greater. See Table V.

With the exception of No. 22, all the buttons contained iron, some of the results being even then too low (Nos. 20 and 21), and others too high (Nos. 14 to 19). Apparently the lime, by entering the slag, assisted in carrying the iron into the tin. That nevertheless some tin was slagged is to be accounted for by the fact that there was not enough iron to prevent it; for metallic iron,[1] although if present in sufficient quantity it will precipitate the tin completely, if in too small amount will only reduce the stannic oxide to stannous oxide, which of course enters the slag.

Kerl[2] mentions the fact, that, by first heating with charcoal alone in

[1] Berthier, Traité des Essais, Vol. II. p. 459.
[2] Metallurgische Probirkunst, 1879, p. 483.

the crucible and then adding the fluxes, the scorification of tin will be lessened. The reason of this is apparent. If the stannic oxide be reduced to the metallic state, the fluxes then added will, upon fusion, simply assist to collect the fine particles of metallic tin into a button, and the danger that stannic or stannous oxide may be carried off in the slag is excluded. But other more powerful causes counteract the good results which might be expected from this modification of the German assay. The most important is the presence of iron in the metal.

TABLE V.

No. of Assay.	Resulting Tin.							
	Total.		In Button.		In Scales.		In Siftings.	
	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.
14	3.456	69.12	3.450	69.00	....	....	....	....
15	3.456	69.12	3.450	69.00	....	....	....	....
16	3.446	68.92	3.440	68.80	....	....	....	....
17	3.446	68.92	3.440	68.80	....	....	Average. 0.006	Average. 0.12
18	3.441	68.82	3.435	68.70	....	....	....	....
19	3.421	68.42	3.415	68.30	....	....	....	....
20	3.391	67.82	3.385	67.70	....	....	....	....
21	3.371	67.42	3.365	67.30	....	....	....	....
22	3.291	65.82	3.285	65.70	....	....	....	....

Although tin is reduced sooner than iron to the metallic state, there is no direct indication by which the assayer can know when to stop with the reduction and when to add the fluxes. If the reduction is carried too far, the iron also will be reduced and will enter the button; if not carried far enough, so that tin still remains unreduced, the main advantage of the modified method is lost. Another drawback is that the fluxes are added to the hot reduced ore in the crucible. This creates a liability to loss of ore by dusting.

In carrying out the experiments the proportions used were the same as with the regular method. The crucibles were placed in the hot fire, and, counting from the time that the ore appeared dark red, it was exposed for a quarter of an hour to a strong heat; then the fluxes were added in their regular order, and the charge heated for one hour.

The behavior in the crucible and the general appearance of crucible and slags were not different from that with the regular method. The color of the salt slag, however, was light green instead of dark purple; that of the borax slag, dark olive-green instead of the usual lighter shade. The tin buttons did not separate well from the slag, as their surface was very uneven. In some places were slight depressions or even cavities; in others, the button was covered with small beads of metal, adhering partly to it and partly to the borax slag. Some of the buttons were bright, and more or less malleable and free from iron; others, however, contained so much iron that they had not the usual white color and bright appearance, and cracked when hammered flat to the size of a dime.

Two double sets of experiments were carried out, the one in a naked crucible at a normal and then at a high temperature, the other in a chalk-lined crucible under the same conditions. The results obtained in the naked crucible are given in Table VI.

TABLE VI.

No. of Assay.	Resulting Tin.							
	Total.		In Button.		In Scales.		In Siftings.	
	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.
23	3.363	67.26	3.33	66.60	....	....	....	....
24	3.333	66.66	3.30	66.00	....	....	....	....
25	3.253	65.06	3.22	64.40	....	....	....	....
Average	3.316	66.32	3.283	65.66	0.002	0.04	0.031	0.62
26	3.484	69.68	3.460	69.20	....	....	....	....
27	3.334	66.68	3.310	66.20	....	....	....	....
28	3.259	65.18	3.235	64.70	....	....	....	....
Average	3.359	67.18	3.335	66.70	....	....	0.024	0.48

The average result of assays Nos. 23, 24, 25, carried out at a normal heat, is over 1 per cent lower than that obtained by the regular method, and there is a difference of 2.6 per cent between the highest and the lowest of the results. The buttons, however, were all free from

iron. Those from assays Nos. 26, 27, 28, carried out at a high temperature, show the presence of iron in quite a marked degree. There was no regularity whatever; No. 26 took up so much iron that the button weighed more than the tin obtained by chemical analysis; Nos. 27 and 28 also took up iron, and at the same time gave some tin to the slag.

The results obtained by this modified method are therefore unsatisfactory. They become still more so if the assay is carried on in the chalk-lined crucible. This can be seen from Table VII.

TABLE VII.

No. of Assay.	Resulting Tin.							
	Total.		In Button.		In Scales.		In Siftings.	
	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.
29	3.419	68.38	3.41	68.20	....	....	....	....
30	3.339	66.78	3.33	66.60	....	....	....	....
31	2.969	59.38	2.96	59.20	....	....	....	....
Average	3.242	64.84	3.233	64.66	0.009	0 18	....	....
32	3.425	68.50	3.390	67.80	....	....	....	....
33	3.365	67.30	3.330	66.60	....	....	....	....
34	3.205	64.10	3.170	63.40	....	....	....	....
Average	3.332	66.63	3.297	65.93	0.001	0.02	0.34	0.68

Of Nos. 29, 30, 31, made at a low temperatnre, one assay (No. 29) contained iron, and between the other two there is a difference of 7.4 per cent. This discrepancy was probably caused by the large amount of tin carried off in the slag of No. 31. Under the caption "Siftings" there is a blank. As siftings there remained in the pan a heavy brown-black slag, which, when examined in the wet way, proved to be rich in tin. The assays Nos. 32, 33, 34, carried on at a high temperature, gave buttons which were all strongly ferruginous; they also showed great irregularity. Judging from the results obtained by the modified method, — Nos. 23 to 32, — the preference must be given to the regular German method, although, as above stated, it would

seem rational to suppose that the loss in tin would be smaller if the black tin were reduced to the metallic state before the flux is added and the fusion begins.

Mitchell[1] gives a method which may be mentioned in connection with the German assay. He mixes 400 grains ore, 100 argol, 300 sodium carbonate, 50 lime, and charges in a crucible so large that it need only be half filled, then gives a cover of sodium carbonate and 200 grains of borax; heats gently, and keeps for at least twenty minutes at a dull red heat, increasing the temperature until tranquil fusion takes place.

The experiments were carried out as directed. A beautiful bottle-green slag resulted. It was as glassy as obsidian, and covered with a thin slag of a lighter green color. The results from three consecutive assays were so discouraging that no further attempts were made in that direction. Nearly all the tin had been taken up by the slag, the only apparent reason being the intimate mixing of ore and flux.

2. *Fusion with Calcium Fluoride.* — There are three methods described under this head, in all of which fluorspar is used as the only or principal flux. They are recommended for ores containing silica only.

Fluorspar is not readily fusible; in fact, it cannot be melted at temperatures produced in a muffle-furnace. Assays requiring it as a flux must therefore be carried on in a pot-furnace. While difficult of fusion, it is liquid when melted, and assists to liquefy compounds which have a refractory character, by holding in suspension the particles which cannot be completely melted. It is therefore very satisfactory if a high temperature is required. Berthier[2] was the first to suggest that a loss in weight takes place if calcium fluoride and silica are heated together. In later publications[3] the formula $2\,CaF_2 + 2\,SiO_2 = SiF_4 + Ca_2SiO_4$ is found with the statement that part of the silica escaped as silicon fluoride. Percy[4] has shown by experiments, that, while a loss in weight does take place, the formula is wrong, as it expresses more than three times the actual amount lost. It would seem, therefore, that the basis on which these methods are founded is not a correct one. Owing to the high temperature, there is always danger of losing tin through volatilization, although[5] in a char-

[1] Manual of Assaying, New York, 1881, p. 481.
[2] Traité des Essais, 1847, Vol. I. p. 436.
[3] E. g. Balling, Metallurgische Chemie, Bonn, 1882, p. 88.
[4] Metallurgy of Fuel, etc., London, 1875, p. 80.
[5] Berthier, Traité des Essais, 1847, Vol. II. p. 458.

coal-lined crucible this may be prevented by entirely excluding the air. In regard to scorification, it seems doubtful whether it can be avoided so as to give satisfactory results.

(1.) Ricketts[1] recommends mixing 10 grams of ore with 10 to 20 grams of fluorspar or cryolite. He then says: "Charge in a charcoal-lined crucible, which is first covered with charcoal and then luted with clay; heat strongly for about one hour; remove carefully from the fire, and tap gently. Treat the button as an alloy afterwards." These instructions were closely followed, fluorspar being used and the time of exposure to heat lengthened, as previous experiments in charcoal-lined crucibles had shown that considerable time is required for the heat to penetrate the lining.

Three experiments were made, resulting in a gray, slightly fritted mass, which contained white tin and black tin mixed with fluorspar. This gave, when crushed in a porcelain mortar and treated with water, gray slimes, and, as residue, the mixture of white and black tin. The complete reduction of the stannic oxide was probably prevented by the intimate admixture of fluorspar. It could scarcely have been caused by insufficient heat. A hot anthracite fire was used; the furnace was 12 × 12 inches, the crucible resting on 3 inches of coal, and being covered by 3 inches of coal (to the lower edge of the flue). The draught was strong, the total time of exposure to heat two hours.

(2.) Ricketts[2] gives a similar charge for a chalk-lined crucible: ore 10 grams, fluorspar 10 grams, powdered charcoal 2 grams, a salt and charcoal cover, and recommends a hot fire.

This method produced negative results. The charge at the bottom and sides of the crucible was fused; it was gray, showing a crystalline structure. At the bottom the fused rim was $\frac{1}{8}$ inch thick, decreasing to $\frac{1}{16}$ inch higher up. Inside this shell was a fritted mass of a color from dark gray to black, consisting apparently of charcoal and fluorspar, with fine particles of tin dispersed through it, and covered by a pumice-like gray slag, probably a mixture of salt and fluorspar.

(3.) Balling[3] recommends mixing 5 grams of ore with 2.5 grams of fluorspar and 2.5 grams of lime, charging the mixture in a charcoal-lined crucible, and treating as in the old German method of assaying iron ores in the dry way; that is, heating for an hour and a half, the last fifteen or twenty minutes at a white heat.

The result of several fusions was a slightly sintered mass of a grayish-

[1] Notes on Assaying, 1886, p. 88.
[2] Ibid., p. 89.
[3] Die Probirkunde, 1879, p. 391.

white color collected into a button. Through it were distributed prills of metallic tin of different sizes, the largest being about the size of a small pin-head. Another negative result.

No pains was spared to make these experiments with fluorspar as thorough as possible, and the failure to obtain satisfactory results must have been due to one of two things; either the ore used was not suited to that mode of treatment, or important particulars were omitted from the directions given.

3. *Fusion with Potassium Cyanide.* — This very valuable method of assaying tin ores is given by Mitchell.[1] In describing his manner of operating, he says he uses a 3-ounce Hessian crucible, rams into its bottom a ½-inch layer of potassium cyanide, adds the charge, consisting of 100 grains of ore mixed with from four to five times its weight of powdered potassium cyanide, and gives a cover of the same flux. He then heats at a moderate temperature for ten minutes, removes the crucible, taps it gently, and allows it to cool. He dissolves the slag in water to see if any reduced metal or heavy particles of the original ore are present, and says that, with ordinary care, the results are accurate to 0.5 per cent.

The basis on which the process rests can be expressed by the formula, $SnO_2 + 2\,KCy = Sn + 2\,KOCy$. Potassium cyanide, being more powerful than any other reagent, acts more quickly, and also at a lower temperature, thus reducing the volatilization of tin to a minimum. The disadvantages are its cost and its very poisonous character. The extra expense need hardly be taken into consideration, as it is very nearly counterbalanced by the saving in time, the difference being fully made up by the superiority of the results obtained. Its poisonous character requires that special care should be taken in handling it. The mortar ought to be covered with a wooden lid, having a hole through which the handle of the pestle passes, and a cloth placed over this to avoid dusting. A sponge or handkerchief should protect nose and mouth while pulverizing, weighing, and mixing the reagent with the ore. The crucible should be placed to cool under a good draught, and care taken to finish the breaking of crucibles and handling of cyanide slags before the fingers have occasion to become moist. If these precautions are observed, there is no danger whatever, as the writer can testify from repeated experience.

In the experiments the ordinary chemically pure potassium cyanide — 98 per cent KCy and 2 per cent KOCy — was first used; then the

[1] Manual of Assaying, 1881, pp. 481–483.

effects produced by the different impurities that are apt to be found in commercial cyanides were studied; and, lastly, the commercial cyanides themselves were tested. The regular method given by Mitchell was varied in different ways, to see what effect modifications from other methods would have on the reagent.

In regard to temperature and time required for the assay, Mitchell says "a moderate fire," which will keep the charge at a "steady fusion" for "ten minutes." It was found that a charge can be kept at a steady continuous fusion for twenty minutes, and the temperature be too low to complete the chemical reaction. Small globules of tin can be seen floating about in the slag, and these will not be collected into one button when the crucible is broken. On the other hand, the temperature may be raised so high that the charge will boil, and the fusion still be steady and continuous. The tin will be reduced, but will not collect into one button, and the assay will again be imperfect. The best way to regulate the temperature would seem to be the following. Begin with a hot fire, and keep it at the highest point to which potassium cyanide can be heated without beginning to boil and evolve heavy fumes. This temperature is easily recognizable after a few assays. Ten minutes is a very good average for the time, beginning when the charge is liquid and brown-red. An assay may be considered finished when the pure upper slag has become so transparent that the impurities contained at the bottom of the crucible are visible through it. The tin will then have collected into one button beneath the lower slag, and very few, if any, prills will be found.

The experiments with potassium cyanide were as follows (1 to 11).

(1.) 5 grams of ore mixed with 20 grams of potassium cyanide were charged in a Battersea crucible, size F, into the bottom of which had been rammed 5 grams of cyanide; the charge was covered with 5 grams of cyanide; time given, ten minutes.

The slag was as liquid as water. When cold, its surface was smooth, and had the shining lustre of porcelain. The crucible was not corroded, and the upper part was free from tin. On breaking the crucible, it was found that the potassium cyanide had filtered through nearly the entire bottom and blackened it, the blackness diminishing on the sides and disappearing near the top. The fracture of the upper slag was coarsely granular; it was opaque, milk-white, brittle, and soluble in water. On the bottom of the crucible was an amorphous, uneven slag; it had a resinous lustre, was subtranslucent, of light green color, harder than the white slag, and insoluble in water. It was charged with the impurities contained in the ore. The button separated well

from the slag, was white, bright, easily cut, malleable, and free from iron.

To examine the slags and lower part of the crucible for prills of tin, they were placed in hot water. The white slag dissolved readily, and the green, as a rule, separated easily from the crucible. The solution of potassium cyanide and cyanate was removed by decanting, and the pieces of crucible taken out and dried apart from the residue in the dish. Both were then crushed separately, and passed through a 60-mesh sieve. The resulting fine pulp was panned, and the siftings weighed with their scales, the slags and crucibles of one series of assays being worked up together, as the weights of the resulting buttons were close enough to justify the proceeding.

TABLE VIII.

No of Assay.	Resulting Tin.							
	Total.		In Button.		In Slags.		In Crucible.	
	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.
35	3.385	67.70	3.330	66.60	....	....	....	....
36	3.365	67.30	3.310	66.20	....	....	....	....
37	3.355	67.10	3.300	66.00	....	....	....	....
38	3.355	67.10	3.300	66.00	....	....	....	....
Average	3.365	67.30	3.310	66.20	0.005	0.10	0.050	1.00

Table VIII. shows that the average result is 0.54 per cent lower than that found by chemical analysis, and the percentage of tin in slags and crucible — 1.10 per cent — rather higher than would have been expected. The greatest discrepancy, however, in the total weights is only 0.03 gram, but expressed in percentage it amounts to 0.6 per cent. It seemed, therefore, more advisable to use 10 grams of ore, as in the next experiments.

(2.) The charge consisted of ore, 10 grams; potassium cyanide, 40 grams; 5 grams cyanide for bottom of crucible, and 5 grams cyanide for cover; crucible, size F; time, fifteen minutes.

The only difference between the first and second assays is, that in the latter the bottom was covered with a larger quantity of green slag, showing (as the crucibles were of the same size as before) a thicker

layer. In assays Nos. 35 to 38 this heavy green slag had collected around the button, filling the space between it and the crucible, and not quite reaching the top of the button, which was covered by the pure white slag. It would, therefore, seem advisable to use, for a 10-gram assay, a crucible having a larger base than the size F of the Battersea make. The button could then lie on the bottom, and be surrounded by the impure slag, the top being covered by pure white slag, which helps reduction and the successful collecting of the metal in one button. If the assays are made in the muffle, the 10- and 20-gram (or Colorado A and B) crucibles, used for lead and slag assays, might be very suitable for the purpose.

TABLE IX.

No. of Assay.	Resulting Tin.							
	Total.		In Button.		In Slags.		In Crucible.	
	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.
39	6.752	67.52	6.685	66.85	....	....	....	....
40	6.752	67.52	6.685	66.85	....	....	....	....
41	6.752	67.52	6.685	66.85	....	....	....	....
42	6.742	67.42	6.675	66.75	....	....	....	....
Average	6.749	67.49	6.682	66.82	0.060	0.60	0.007	0.07

The total average differs here only 0.35 per cent from the chemical analysis ; three assays agree perfectly, and the fourth shows only 0.01 gram, or 0.1 per cent difference, which is very close. The percentage of tin in slags and crucible is half as much as that in the previous assays (Table VIII.), suggesting that the first assays, Nos. 35 to 38, might have been benefited by a longer exposure to the heat.

(3.) In assaying ores in the dry way, it has been, and still is, very common to give a salt cover. To try what effect this would have, the foregoing two sets of experiments were repeated, adding the salt.

It was found that the charges melted less rapidly and were not so thin as before; when cold, the surface of the slag was uneven and rough ; the crucible was corroded, but less soaked with potassium cyanide, the upper part showing no prills of tin; the slag showed a very coarsely granular structure; it was opaque, milk-white, but some-

what dull in comparison with that of the pure cyanide. No other differences were observable.

The low results shown by the averages were startling. They could not be attributed to any action of the sodium chloride, nor to the small amount of silicate derived from the corrosion of the crucible. The salt was examined, and found to be rich in sulphuric acid; this explained the loss in tin (see Table XVI.). Fine Michigan table-salt was then tested, and sulphuric acid found to be present here also; therefore, nothing further was attempted with salt. No special advantage is apparent in its use with potassium cyanide, as the slags are thinner without it, and

TABLE X.

No of Assay.	Resulting Tin.							
	Total.		In Button.		In Slags.		In Crucible.	
	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.
43	3.266	65.32	3.200	64.00	....	....	....	....
44	3.231	64.62	3.165	63.30	....	....	....	....
45	3.226	64.52	3.160	63.20	....	....	....	....
46	3.176	63.52	3.110	62.20	....	....	....	....
Average	3.225	64.49	3.159	63.17	0.020	0.40	0.046	0.92
47	6.505	65.05	6.430	64.30	....	....	....	....
48	6.405	64.05	6.330	63.30	....	....	....	....
49	6.235	62.35	6.160	61.60	....	....	....	....
50	6.220	62.20	6.145	61.45	....	....	....	....
Average	6.341	63.41	6.266	62.66	0.065	0.65	0.010	0.10

form a better "wash" for the sides of the crucible than the less readily fusible salt. If it is to be used at all, recourse must be had to Ohio salt, which, according to the Report of the Geological Survey of Ohio, "Economic Geology," 1888, Vol. VI. p. 663, is free from sulphates, while Michigan salt always contains them.

(4.) Ricketts recommends the chalk-lined crucible for the assay with potassium cyanide. He gives the charge, 10 grams of ore and 40 grams of potassium cyanide, with half of the flux in the bottom of

the crucible and the other half mixed with the ore, then the usual cover of cyanide, to which is added a salt cover, and heats for fifteen minutes. These directions were followed, only omitting the salt cover.

It was expected that the chalk-lining would at least partly prevent the cyanide from filtering into the crucible, but no difference could be observed, the crucible being just as black as if it had had no chalk-lining.

The structure of the slag was slightly coarser; it was more uneven in fracture, and of a bluish gray color, instead of the usual pure milk-white.

The average of the results is 2.1 per cent lower than that obtained by the chemical analysis, and 1.75 per cent lower than the average of the corresponding 10-gram assays, Nos. 39 to 42. The separate assays also show a difference of 1.9 per cent, which is very unfavorable. The first reason for this that suggested itself was the possible presence of gypsum in the chalk, but no sulphates were found. There remains, therefore, only the presence of lime to account for the slagging of tin.

TABLE XI.

No. of Assay	Resulting Tin.							
	Total.		In Button.		In Slags.		In Crucible.	
	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.
51	6.648	66.48	6.625	66.25	....	....	0.017	0.17
52	6.618	66.18	6.595	65.95	....	....	0.017	0.17
53	6.458	64.58	6.435	64.35	....	....	0.017	0.17
54	....	....	....	....	....	....	....	....
Average	6.574	65.74	6.551	65.51	0.006	0.06	0.017	0.17

By a mistake in assay No. 54, the entire 40 grams of potassium cyanide were mixed with the 10 grams of ore, and charged with it. No button whatever was obtained. The bottom of the crucible was covered with a spongy, greenish gray mass, and this by the ordinary white slag of potassium cyanide. This shows the necessity of having a good layer of potassium cyanide on the bottom of the crucible, probably on account of the tendency the tin has not to collect in a single button. If the crucible is filled with a uniform mixture of ore and

cyanide, a number of small buttons will be produced mixed with the slag. If a layer of pure potassium cyanide cover the bottom of the crucible, the particles of tin will sink into it, and form on the undisturbed bottom a nucleus, around which other particles can collect, and thus take up gradually most of the tin. The effect is similar if charcoal be mixed with the potassium cyanide (see 5 to 7).

(5.) The regular method of assay with potassium cyanide was modified in the same way as the German method; i. e. the stannic oxide was first reduced by charcoal, and the potassium cyanide then added to collect the particles of metallic tin into one button. It was expected that the iron would be taken up by the potassium cyanide instead of alloying with the tin. The result was tested by dissolving the slag in water, filtering the solution, acidulating the filtrate with hydrochloric

TABLE XII.

No. of Assay.	Resulting Tin.							
	Total.		In Button.		In Slags.		In Crucible.	
	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.	Grams.	Per cent.
55	3.223	64.46	3.100	62.00	....	....	....	....
56	3.202	64.04	1.310	26.20	1.892	37.84	See slags.	See slags.
57	3.198	63.96	3.075	61 50	....	....	....	....
58	3.198	63.96	3.075	61.50	....	....	....	....
Average	3.205	64.11	....	....	0.033[1]	0.66[1]	0.090[1]	1.80[1]

acid, and boiling to expel the hydrocyanic acid formed. It was then tested with a mixture of ferrous and ferric salt, which gave the reactions for potassium ferro- and ferri-cyanide. Another part of the filtrate was evaporated to dryness, treated with nitro-hydrochloric acid, again evaporated to expel all cyanogen, and taken up with hydrochloric acid. On adding ammonium hydroxide, the reddish-brown ferric hydroxide was precipitated, showing again the presence of iron.

In the experiments, 5 grams of ore were mixed with 1 gram of charcoal, placed in a naked crucible, size F, and heated for a quarter of an hour after the charge had become dark red; then 20 grams of potassium cyanide were added; after this thirteen minutes were required before quiet fusion was obtained.

[1] Except No. 56.

The slags had the usual characteristics of the pure cyanide, but black patches of carbon were irregularly dispersed through them. The buttons were white, very bright, malleable, and free from iron; they were however rough, and covered with bead-like excrescences of tin.

These results are very unsatisfactory. The average gives 3.73 per cent less tin than is present; it is 3.20 per cent lower than that of the corresponding 5-gram assays, Nos. 35 to 38, and 2.21 per cent lower than that of the modified German method, Nos. 23 to 25. On comparing the separate weights of the tin-buttons obtained, it will be seen that the tendency of the tin not to collect in one button is enhanced by the presence of the charcoal with which the ore had been mixed. The only result of positive interest obtained by the experiments is the fact, that the iron of the black tin, reduced to the metallic state, goes into the slag, and not into the button, as in the modified German assay.

Similar experiments carried on in a charcoal-lined crucible were equally unsatisfactory.

(6.) The same method, only using a porcelain crucible, bears the name of Levol's method. Kerl[1] and Balling[2] give it as follows: 2 grams of ore are mixed with 20 per cent of charcoal in a porcelain crucible, and heated for from fifteen to thirty minutes in a muffle; 0.50 to 0.75 gram of potassium cyanide is added, and the heating continued another five minutes. They say that the results are accurate to from 0.3 to 0 5 per cent.

It was found that it is necessary to grind ore and charcoal very fine, and that an excess of charcoal must be avoided.

Two grams of ore were mixed with 0.5 gram of charcoal, and heated for half an hour in the muffle at a good scorification temperature. The crucibles were then removed, allowed to cool, 1 gram of potassium cyanide added, and the crucibles returned to the muffle for ten minutes.

They were all blackened and somewhat corroded; the slag was of a light gray color, from the charcoal, and there was a small amount of green slag. In none of the assays had the tin collected in one button; it varied in size from a button down to a fine gray powder. The crucibles were now placed in hot water, the small buttons picked out, and the fine tin separated from the charcoal and insoluble green slag by washing. This is the uncertain part of the assay. The fine tin

[1] Metallurgische Probirkunst, 1882, p. 483.
[2] Die Probirkunde, 1879, p. 392.

separates with difficulty from the green slag. If this is entirely removed, some tin will be lost, and the resulting weight will be too low; if all the tin is recovered, some slag remains with it, and the result is too high. Thus the assayer must choose between two evils, and this causes a feeling of uncertainty. Any slight mistake is multiplied by fifty when the result is expressed in percentage. This is illustrated in Table XIII.

TABLE XIII.

No. of Assay.	Resulting Tin.	
	Grams.	Per cent.
59	1.350	67.50
60	1.335	66.75
61	1.305	65.25
Average	1.330	66.50

The average is too low, showing loss from excessive care in washing to obtain pure tin. The greatest discrepancy between the single results is 2.25 per cent. This shows how difficult it is to effect a satisfactory separation of fine insoluble slag and fine tin. During the past two years the writer has made a number of assays according to this method. The results obtained from duplicate assays only agreed well when all the tin had collected in one or more large buttons; they were never satisfactory if the tin was present in the form of fine powder. It was not discovered why in one case a button, in another a powder, should be obtained. The statement made by Kerl and Balling, that the results of this method are accurate within 0.5 per cent, needs modification, as it certainly does not apply to all tin ores. Probably those of the Black Hills contain impurities that were not present in the ores Kerl and Balling have reference to.

(7.) The last experiments made with potassium cyanide followed T. W. E. David, in his "Report on the Geology of the Vegetable Creek Tin-Mining Field," Sydney, New South Wales, 1887, p. 153. Mr. David says that at the Glen Smelting Works at Tent Hill the potassium cyanide is mixed with charcoal. The only reason that can be brought forward for this modification is, that the potassium cyanide used must have been very impure. Thus, in order to increase its reducing power, charcoal was added.

Five grams of ore were fused with a mixture of 30 grams of potassium cyanide and 1 gram of charcoal; only two thirds of the flux were mixed with the ore, one third being rammed into the bottom of the naked crucible.

Contrary to expectation, the charge fused as readily as if it contained no charcoal, and came very quickly to a quiet fusion. On breaking the crucible, no button was to be found, but only fine prills of tin disseminated through a pumice-like dark gray slag, containing irregular streaks colored black by carbon. This shows again the necessity, alluded to above, of having in the bottom of the crucible a layer of pure potassium cyanide for the reduced particles of tin to collect in and form a button.

In the preceding experiments, the ordinary c. p. potassium cyanide was used, as it seemed important that the results obtained should be based on a reagent whose composition was definitely known. But this pure cyanide is somewhat expensive ($1.90 a pound), and, to see whether its use was absolutely necessary, the different commercial grades were tested. First, however, c. p. cyanide was mixed with cheaper fluxes, such as pure potassium and sodium carbonate, to obtain, if possible, a flux which would have the same action as the pure cyanide, but be considerably cheaper. The effect produced by potassium sulphate was also investigated.

(8.) The effect of potassium carbonate on the cyanide assay was tested by fusing in a naked crucible 5 grams of ore with 30 grams of potassium cyanide, to which had been added different quantities of this flux. One third of the flux mixture was rammed into the bottom of the crucible, and the rest mixed with the ore, except a small quantity kept to be used as a cover. The average time required to complete the assay was ten minutes from the time the charge began to fuse.

The charges behaved during the fusion just as if pure cyanide had been used. The crucibles remained uncorroded. On breaking, it was found that with the increase of potash in the mixture the crucible was less soaked with cyanide; in the last assay, with 41.17 per cent potassium carbonate, the bottom was blackened only to the depth of ⅛ inch. The top of the slag was, in all the assays, as smooth as with pure cyanide. The color had, however, changed; with small quantities of potash, the slag, on the surface as well as on the fracture, had a bluish tinge, which grew deeper, until, in the last assay, it became of a decided blue-gray color. The structure of the slag became more and more granular, until it was so coarse as to have the appearance of granulated sugar cemented together. Finally, with the increase of potash the

lustre of the slag decreased, while at the same time it grew harder and tougher. The buttons of tin obtained did not differ in appearance from those with pure cyanide.

TABLE XIV.

No. of Assay.	Charge.				Resulting Tin.	
	Ore.	KCy.	K_2CO_3.			
	Grams.	Grams.	Grams.	Per ct. in mixture.	Grams.	Per cent.
62	5	30	3	9.09	3.325	66.50
63	5	30	6	16.66	3.300	66.00
64	5	30	9	23.07	3.275	65.50
65	5	30	12	28.57	3.263	65.26
66	5	30	15	33.33	3.235	64.70
67	5	30	18	37.50	3.230	64.60
68	5	30	21	41.17	3.190	63.80

In Table XIV. two points are striking: first, that the amount of tin obtained grows smaller with the increase of potassium carbonate; and, secondly, that no tin is recovered in the slags or in the crucible. Both facts are probably due to the same cause,[1] namely, that metallic tin decomposes alkali carbonates, forming stannates which are not readily fusible. In fact, Berthier[2] says: "By fusing one part of metallic tin with four or five parts of potassium carbonate, a substance is obtained which is liquid, compact, opaque, yellow like wax, and crystalline." In the assays given in the table, the quantity of flux was increased in proportion to the percentage of potash added. As this alone might have had a bad effect on the result, a number of assays were made in which the percentage of potash was the same as in Nos. 64 to 68, the total amount of flux, however, not exceeding 30 grams. They were all very unsatisfactory. Over fifteen minutes were required for the fusion, and there was no regularity in the results. To counteract this, assays were made, to which charcoal had been added to strengthen somewhat the weakened action of the diluted fluxes, placing, however, fluxes free from charcoal in the bottom of the crucibles. The buttons gave higher results, but the irregularity in their weights was the same as before. It

[1] Berthier, Traité des Essais, 1847, Vol. II. p. 459.

[2] *Op. cit.*, Vol. I. p. 445.

would appear, therefore, that the potassium cyanide needs to be practically free from potassium carbonate; 9.09 per cent of the latter showing already a loss of 1.34 per cent in the result obtained.

(9.) The effect of sodium carbonate was tried in the same way. Although this alkali is not common with potassium cyanide, it was thought advisable to test it, as, when potassium cyanide is used in blowpipe experiments, it is generally mixed with an equal weight of sodium carbonate.

The mixtures were not so readily fusible as the corresponding potassium charges, and required fifteen minutes, instead of ten as before. They were, however, just as thin when once melted. When cold, the slags were similar to the potassium slags, only more vitreous and harder. The bluish gray shades of color were missing; the slags were white, but had no lustre whatever; the buttons were, on the whole, the same as above, but showed bead-like excrescences.

TABLE XV.

No. of Assay.	Charge.				Resulting Tin.	
	Ore.	KCy.	Na_2CO_3.			
	Grams.	Grams.	Grams.	Per ct. in mixture.	Grams.	Per cent.
69	5	30	3	9.09	3.315	66.30
70	5	30	6	16.66	3.280	65.60
71	5	30	9	23.07	3.270	65.40
72	5	30	12	28.57	3.110	62.20
73	5	30	15	33.33	3.055	61.10
74	5	30	18	37.50	3.025	60.50
75	5	30	21	41.17	2.910	58.20

Table XV., it will be seen, contains no columns for tin in slags or crucible, and the percentage of tin extracted is smaller than with the potassium salt, the inevitable conclusion being that an admixture of soda has a still more deleterious effect on the result than one of potash.

(10.) The influence of potassium sulphate is a very bad one. Bloxam[1] states that, in the presence of potassium sulphate, stannous

[1] Journal of the Chemical Society, New Series, 1865, Vol. III. p. 97.

and stannic sulphides are formed; the former if the proportion of sulphate be small, the latter if sufficiently large, the stannous sulphide remaining undissolved as a black powder if the slag be treated with water, the stannic sulphide going into solution, and being precipitated as yellow sulphide upon acidulating.

In the experiments, different amounts of potassium sulphate were added to 30 grams of potassium cyanide, and two thirds of the new flux mixed with the ore, one third being placed in the bottom of the naked crucible. The time required for tranquil fusion was ten minutes.

The charges fused readily, and the crucibles were not attacked; the surface of all the slags was smooth, the color deepening with the percentage of potassium sulphate from light red, with a dark rim, to black. The fracture was at first similar to that of pure cyanide, and grew finer until it became stony; the color, at first very light peach-blossom, became darker, till it reached, with the last assay, a dark flesh-color. The hardness and toughness of the slags also increased with the percentage of potassium sulphate.

TABLE XVI.

No. of Assay.	Charge.				Resulting Tin.	
	Ore.	KCy.	K_2SO_4.			
	Grams.	Grams.	Grams.	Per ct. in mixture.	Grams.	Per cent.
76	5	30	0.3	0.99	3.185	63.70
77	5	30	0.6	1.96	No button.	No button.
78	5	30	1.0	3.22	3.060	61.20
79	5	30	2.0	6.25	2.525	50 50
80	5	30	3.0	9.09	2.490	49.80
81	5	30	4.0	11.76	2.435	48.70
82	5	30	5.0	14.28	2.250	45.00

The results obtained show a gradual decrease in the percentage of tin. One per cent of potassium sulphate causes already a loss of 4.14 per cent tin. In assay No. 76 some stannous sulphide was present; it decreased in No. 77, and was absent in No. 78, which, when acidulated, gave a copious precipitate of stannic sulphide. The deleterious influence of potassium sulphate, which was first shown by Bloxam, is thus verified by a series of systematic figures.

TABLE XVII.

No. of Assay.	Granular Purified. Resulting Tin.		No. of Assay.	Pure for Gold Platers. Resulting Tin.		No. of Assay.	Common Fused. Resulting Tin.		No. of Assay.	Common for Mining Purposes. Resulting Tin.	
	Grams.	Per ct.		Grams.	Per ct.		Grams.	Per ct.		Grams.	Per ct.
83	3.225	64.50	86	3.370	67.40	89	3.220	64.40	92	3.115	62.30
84	3.225	64.50	87	3.355	67.10	90	2.990	59.80	93	3.010	60.20
85	3.100	62.00	88	3.340	66.80	91	2.905	58.10	94	2.900	58.00
Aver.	3.183	63.66	Aver.	3.355	67.10	Aver.	3.038	60.76	Aver.	3.008	60.16

(11.) Various grades of the commercial potassium cyanide were obtained and tested. The grade of the cyanide can be judged from its general appearance, the pure being coarsely crystalline, soft, and snow-white. The lowest grade — " or mining purposes " — is reached when the structure becomes stony, and strongly resists pulverization; the color is a decided gray, resulting from finely divided iron.

In each experiment 20 grams of the special brand of cyanide were mixed with 5 grams of ore, 5 grams being reserved for the bottom of the crucible, and 5 grams for a cover. The average time required for tranquil fusion was ten minutes.

a. " Granular Purified," — 86 to 90 per cent KCy, with some KOCy and K_2CO_3, — $1.25 per pound.

The upper slag did not have the beautiful crystalline structure nor the strong lustre of the pure cyanide. It was white, but showed a bluish tinge near the top. The average result is 4.18 per cent too low, and the greatest discrepancy between the single results is 2.50 per cent.

b. " Pure for Gold Platers," — 92 per cent KCy, 5 per cent KOCy, and 3 per cent other salts, — $1.25 per pound.

The general characteristics are the same as those of Nos. 83 to 85, but the results are higher and more uniform.

c. " Common Fused," — 85 per cent KCy, — $0.55 per pound.

The upper slag has a stony fracture, and is of a dull white color. The results are very low and very irregular.

d. " Common for Mining Purposes," — 65 per cent KCy, — $0.50 per pound.

The surface of the slag is smooth, has a dark blue color with gray and yellow streaks passing through it. The fracture is stony, the slag is hard and tough, and is flesh-colored. The results are as bad as those from Nos. 89 to 91.

It will be seen that, of the different grades, only the brand "Pure for Gold Platers" produces results — 0.74 per cent too low — which are sufficiently accurate for every-day purposes. As these brands represent each a certain definite grade of a particular firm, it will be seen that the assayer is obliged to test each separately, making the ordinary chemically pure potassium cyanide — 98 per cent KCy and 2 per cent KOCy — his standard.

THE ENGINEERING LABORATORIES.

BY GAETANO LANZA, PROFESSOR OF APPLIED MECHANICS, IN CHARGE OF THE DEPARTMENT OF MECHANICAL ENGINEERING.

IN the "Technology Quarterly" for May, 1888, an article was published which gave the history of the Engineering Laboratories up to within two years of the present time. Before February 1, 1890, they occupied a little more than one half the basement of the Rogers Building; and inasmuch as that space was already pretty well filled at the time the article was written, it may be said to represent fairly well their condition up to the last mentioned date. The number of students who are now using these laboratories, however, is so large, that it would not be possible to accommodate them all in the Rogers Building. Hence a new Engineering Building has been erected on Trinity Place, not far from the other buildings of the Institute; and it is the object of the present article to describe this building and the present condition of the Engineering Laboratories.

The building covers a ground area of 52 feet by 148 feet, and is six stories high; the two lower floors being entirely devoted to the Engineering Laboratories, while the two middle floors are occupied by the drawing-rooms and recitation-rooms of the Mechanical Engineering Department, and the two upper floors by the drawing-rooms and recitation-rooms of the Civil Engineering Department.

The general style of the building is that of the modern mill, a row of cast-iron columns extending lengthwise through the middle of each floor, at distances of eight feet, centre to centre.

A pair of yellow-pine beams bolted together about one inch apart extends from each column to each lateral wall, the spans being about 24 feet.

The beams supporting the basement floor are each 11 inches by 18 inches; those supporting the first floor, 10 inches by 18 inches; those supporting the second, 7 by 16 inches; and above that, 6 by 16 inches. In the basement the under flooring is 4 inches thick, and on all the other floors 3 inches thick, while the top floors are all $\frac{7}{8}$ inch thick.

The wall on the south side is like that of a pilastered mill, while that on the north side is more architectural.

It will thus be seen that the aim has been to construct a building which, while containing heavy machinery in operation, should be sufficiently free from vibration to render it possible to make experiments requiring delicate measurements.

Another engineering feature which ought to be mentioned is that the building does not contain any boilers, but that the steam for heating and also for power is furnished from the boilers in the basement of the Rogers Building, whence it is conveyed through a six-inch pipe about 1,000 feet long, buried under the ground, and protected by being wrapped in two thicknesses of asbestos and inserted in a wood log.

The heating system, which is partly direct and partly indirect, was designed by Mr. S. H. Woodbridge, and the ventilation is obtained by means of a Sturtevant blower acting in connection with the indirect portion of the system.

Automatic valves are used almost entirely on the radiators, so that the steam supply to any radiator is regulated automatically by the temperature of the room.

These laboratories are now called the Engineering Laboratories, and the building is called the Engineering Building, because it is especially devoted to the engineering work of the school, both the general and the special. Thus, in its recitation-rooms are taught the classes in mechanism, in thermo-dynamics and steam engineering, in hydraulics, and in strength of materials, all of which may be called general engineering studies, as all these subjects are taught, to a greater or less extent, to the students of civil, of mechanical, of mining, of chemical, and of electrical engineering. Besides this, all the drawing-room work of the students of these courses is done in this building, and all the purely professional work of the civil and mechanical engineering courses is carried on here; this including practically all the *engineering* work proper of the above stated courses. Hence it follows that it is the building where the purely engineering work is done for all departments of the school.

The Laboratories are really an aggregation of the following: —

1. A laboratory devoted to experimental work upon the strength and other resisting properties of materials used in construction.
2. A laboratory of steam engineering.
3. An hydraulic laboratory.
4. A laboratory where other engineering experiments are made, but which is not yet sufficiently differentiated to be divided into its component parts.

The objects to be accomplished by these laboratories are the following: —

First. To give the students practice in such experimental work as any engineer is constantly liable to be called upon to perform in the practice of his profession; as boiler tests, engine tests, power determinations, etc.

Second. To give the students some experience in carrying on original investigations in engineering subjects with such care and accuracy as to render the results of real value to the engineering community.

Third. By publishing from time to time the results of such investigations, to add gradually to the common stock of knowledge.

The two lower floors of the building are entirely devoted to the Engineering Laboratories, thus increasing their capacity from about 5,550 square feet, as in the Rogers Building, to about 13,900 square feet. Cuts of these laboratories are shown here, and the following statement of the apparatus they contain is copied from the twenty-fifth Catalogue of the Institute: —

"The laboratory for testing the strength of materials is furnished with the following apparatus. An Olsen testing machine of 50,000 pounds' capacity, for determining tensile strength, elasticity, and compressive strength. A testing machine of the same capacity for determining the transverse strength and stiffness of beams up to 25 feet in length, and of framing-joints used in practice. Machinery for the measurement of the strength, twist, and deflection of shafting while running and under the conditions of practice. Machines for time tests of the transverse strength and deflection of full-sized beams; for testing the tensile strength of mortars and cements, and of ropes; for testing the effect of repeated stresses upon the elasticity and strength of iron and steel; for determining the strength and elasticity of wire; for determining the deflection of parallel rods when running under different conditions. Also accessory apparatus for measuring stretch, deflection, and twist.

"The hydraulic laboratory contains, — A closed tank 5 feet in diameter and 27 feet high, connected with a stand-pipe 10 inches in diameter and about 90 feet high. Apparatus, in connection with the tank and stand-pipe, for making experiments on the flow of water through orifices and mouth-pieces, over weirs and in pipes, under different heads and under different conditions, and on the losses of head occurring under different circumstances. A 6-inch Swain turbine, so arranged that it can be run under different heads, and that measurements can be made of the power exerted, of the efficiency, etc., under different gates.

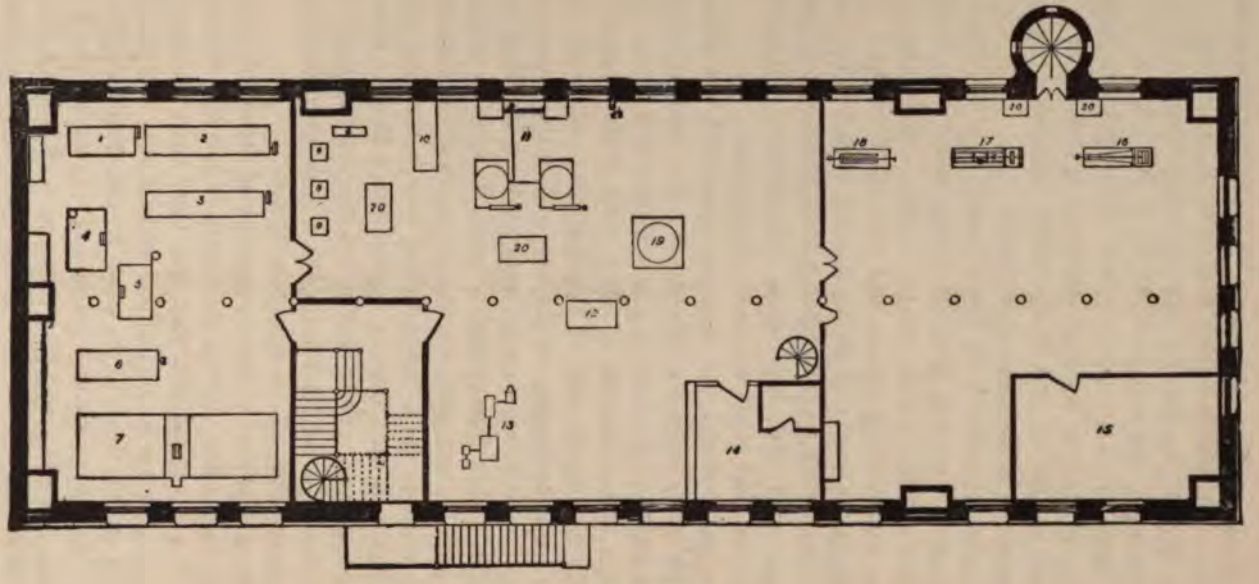
"The steam laboratory contains, — A triple expansion engine, with cylinders of 9 inches', 16 inches', and 24 inches' diameter respectively, and 30 inches' stroke, arranged in such a way as to be run single, compound, or triple, as desired for the purposes of experiment. This engine is of the Corliss type, and was built by E. P. Allis & Co. It will have a capacity of about 150 horse-power when running triple, with an initial pressure of 150 pounds in the high pressure cylinder. It is connected with a surface condenser and all the other apparatus necessary to adapt it to the purposes of accurate experiment.

"This laboratory also contains a 16 horse-power Harris-Corliss engine, and an 8 horse-power engine, used for giving instruction in valve-setting, etc. It is also equipped with several surface condensers, steam pumps, calorimeters, mercurial pressure and vacuum columns; apparatus for determining the quantity of steam issuing from a given orifice or through a short tube under a given difference of pressure; apparatus for testing injectors; and with indicators, planimeters, gages, thermometers, anemometers, and other accessory apparatus.

"The engineering laboratories are also provided with a number of friction brakes; with machinery for determining the tension required in a belt or rope to enable it to carry a given power at a given speed, with no more than a given amount of slip; with three transmission dynamometers; with a complete set of Westinghouse air-brake apparatus, including the parts belonging on the car and on the locomotive; with cotton machinery as follows, namely, two cards, a drawing frame, a speeder, a fly frame, a ring frame, and a mule, as well as accessory apparatus. There are also available for the purposes of experiment, in connection with the work of these laboratories, two horizontal tubular boilers, one large Babcock and Wilcox boiler, and a Porter-Allen engine of about 80 horse-power, all situated in the Rogers Building; also another boiler, a 40 horse-power Brown engine, a number of looms, and other apparatus in the workshops on Garrison Street."

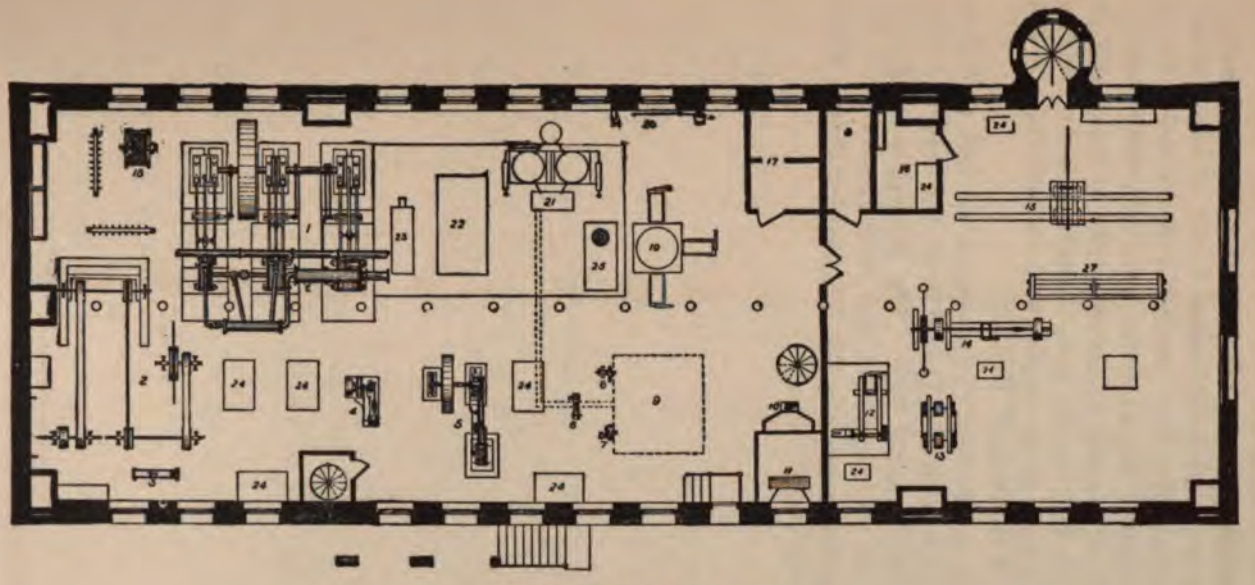
The most important addition to the equipment of these laboratories is that of the triple expansion engine, inasmuch as it is the first triple expansion engine of a practical size that has ever been arranged for making experiments; and by its means the laboratories are placed in a position which will enable them to do work for the triple engine of a character similar to that done for the compound engine by the United States Naval Engineers in 1874, and also to make such researches with a triple or a compound engine as were made upon single engines by Hirn, Hallauer, and others.

The hydraulic tank and stand-pipe places this laboratory in a posi-



FIRST FLOOR.

1. Drawing-Frame.
2. Speeder.
3. Fly-Frame.
4. Carding Engine.
5. Carding Engine.
6. Spinning Frame.
7. Mule.
8. Calorimeters.
9. Calorimeters.
10. Calorimeters.
11. Injector and Weighing Tanks.
12. Tank for Turbine-Wheel.
13. Westinghouse Air Brake.
14. Office.
15. Office.
16. Olsen's Fifty-Thousand Pounds Tension Machine.
17. Repeated-Bending Machine.
18. Cement-Testing Machine.
19. Tank for Hydraulic Experiments.
20. Tables.
26. Mercury Column.



BASEMENT, 150 × 50 FEET.

1. Triple Expansion Engine, 150 H. P.
2. Rope and Belt Transmission Testing Machine.
3. Engine Lathe.
4. Kendall Engine, 8 H. P.
5. Harris-Corliss Engine, 16 H. P.
6. Steam Pump.
7. Rotary Pump.
8. Centrifugal Pump.
9. Tank.
10. Fan Engine.
11. Steam Pipes for Heating Air.
12. Rope-Testing Machine.
13. Parallel-Rod Machine.
14. Shaft-Testing Machine.
15. Fifty Thousand Pounds Beam-Testing Machine.
16. Cement Room.
18. Dynamo, 500 Lights.
19. Wrought-Iron Tank for Hydraulic Experiments (5 feet diameter, 27 feet high).
20. Hydraulic Stand-Pipe (10 in. diam., 85 feet high).
21, 22. Cisterns.
23. Condenser.
24. Tables.
25. Turbine-Wheel and Tank.
26. Mercury Column.
27. Apparatus for Time Test of Wooden Beams.

tion to perform some work of great value in hydraulic experiments which would be impossible without such apparatus. The laboratory with its present equipment furnishes the means, — 1st, of accommodating the number of students that now need this instruction, with an opportunity for some growth; 2d, of giving good laboratory instruction to the students; 3d, of carrying on investigations of importance in the engineering line. All this can be done, inasmuch as the building is adapted to the purposes of an engineering laboratory, — a fact which was never true of the Rogers Building.

The two middle floors are devoted to drawing-rooms and recitation-rooms for the Mechanical Engineering Departments, the drawing-rooms being well arranged to furnish good light. The two upper floors contain, as has been said, the drawing-rooms and recitation-rooms of the Civil Engineering Department, and also the Engineering library, which is common to both departments.

THE VELOCITY OF BODIES OF DIFFERENT SPECIFIC GRAVITY FALLING IN WATER.[1]

BY ROBERT H. RICHARDS, PROFESSOR OF MINING AND METALLURGY, AND A. E. WOODWARD, S.B.

IN Rittinger's "Aufbereitungskunde" of 1867 occurs (p. 195) the following table, which shows the rate of falling in water of fragments of minerals of irregular shapes and of five different specific gravities: —

		Diameter of Particles in Millimeters.							
		10	8	6	4	3	2	1	.5
	Sp. Gr.	Velocity in Meters per Second.							
Auriferous Silver	15.0	.90	.80	.70	.57	.50	.40	.29	.20
Galena	7.5	.62	.55	.48	.39	.34	.28	.20	.14
Pyrites	5.0	.49	.43	.38	.30	.26	.22	.15	.10
Quartz	2.6	.30	.27	.24	.19	.17	.14	.10	.07
Coal	1.3	.13	.12	.10	.08	.07	.06	.04	.03

With the purpose of extending this valuable table to cover a larger number of minerals and sizes of each mineral, and of making it more easy of application in deciding upon a sieve-scale for any group of minerals, we have prepared the curves of velocity of fall of bodies in water shown in Plates I., II., III., and IV., using Rittinger's formula

$$V = 2.44 \sqrt{D(\delta - 1)};$$

where V = velocity in millimeters per second,
" D = diameter of particles in meters,
" δ = specific gravity of the mineral.

These curves embrace a large number of specific gravities, and for each specific gravity the relation of the diameter of the mineral particle to the velocity of its fall in water is given for all diameters and velocities used in practice.

The second diagram is an enlargement of the lower left-hand corner of the first, the third bears the same relation to the second, and in like manner the fourth to the third. Plate I., therefore, covers the work

[1] Presented at a meeting of the American Institute of Mining Engineers, Washington, D.C., February, 1890.

Substance.	Sp Gr. δ.	Substance.	Sp. Gr. δ.
Mineral Coal	1.0 – 1.8	Stibnite	4.5
Gypsum	2.3	Tetrahedrite	4.5 5.1
Chrysocolla	2.0 2.2	Bornite	4.4 5.5
Graphite	2.09	Marcasite	4.68 4.85
Quartz	2.5 2.8	Pyrolusite	4.8
Orthoclase	2.44 2.6	Greenockite	4.8 5.0
Serpentine	2.5 2.65	Pyrite	4.8 5.2
Oligoclase	2.56 2.72	Menaccanite	4.5 5.0
Calcite	2.5 2 78	Hematite	4.5 5.3
Labradorite	2.67 2 76	Millerite	4.6 5.65
Talc	2.57 2.8	Magnetite	4.9 5.2
Pyrophyllite	2.75 2.92	Franklinite	5.07
Muscovite	2.75 3.1	Miargyrite	5.2 5.4
Anhydrite	2.9	Proustite	5.4 5.6
Dolomite	2.8 2.9	Chalcocite	5.5 5.8
Margarite	2.99	Zincite	5.4 5.7
Cryolite	2.9 3.07	Cerargyrite	5.55
Amphibole	2.9 3.4	Pyrargyrite	5.7 5.9
Apatite	2.9 3.25	Cuprite	5.85 6.15
Tourmaline	2.9 3.3	Scheelite	5.9 6.1
Fluorite	3.0 3.25	Cobaltite	6.0 6.3
Forsterite	3.3 3.5	Arsenopyrite	6.0 6.4
Pyroxene	3.2 3.5	Tellurium	6.1 6.3
Epidote	3.25 3.5	Anglesite	6.1 6.4
Fowlerite	3.4	Wulfenite	6.0 7.0
Rhodonite	3.4 3.68	Polybasite	6.2
Rhodochrosite	3.4 3.7	Stephenite	6.27
Azurite	3.5 3.8	Cassiterite	6.4 7.1
Diamond	3.53	Smaltite	6.4 7.2
Calamine	3.16 3.9	Cerussite	6 5
Garnet	3.15 4.3	Pyromorphite	6.5 7.1
Siderite	3.7 3.9	Wolframite	7.1 7.55
Limonite	3.6 4.0	Argentite	7.19 7.36
Malachite	3.7 4.0	Galenite	7.25 7.7
Atacamite	3.8	Niccolite	7.3 7.7
Sphalerite	3.9 4.2	Sylvanite	7.99 8.3
Corundum	3.9 4.2	Copper	8.84
Willemite	3.9 4.18	Cinnabar	9.0
Tephroite	4.0 4.1	Silver	10.1 11.1
Smithsonite	4 0 4.5	Amalgam	10.5 14.0
Chalcopyrite	4.1 4.3	Palladium	11.3 11.8
Chromite	4.3 4.57	Mercury	13.57
Barite	4.3 4.7	Gold	15 6 19.5
Enargite	4.4 4.45	Platinum	16.0 19.0
Pyrrhotite	4.4 4.7	Iridosmine	19.3 21.12
Molybdenite	4.4 4.8	Platiniridium	22.6 23.0

of the coarsest jigging 2 inches in diameter; and Plate II. covers from $\frac{1}{2}$ inch downward; Plate III., from $\frac{1}{10}$ inch downwards; Plate IV., from $\frac{1}{60}$ inch downwards.

We have compiled a table, which appears on the opposite page, of most of the minerals likely to demand water separation, using the specific gravities given in Dana's "System of Mineralogy."

The diagrams may be used in determining, for any group of minerals to be separated, a sieve-scale; that is to say, a list of sieves arranged in series in such manner that the sands treated by them shall be suitably prepared for jigging. Rittinger (p. 224) adopts as the most satis-

	Rittinger's Sieve-scale (p 224).	Rittinger's Fall Velocities (p. 271). Velocity of Fall in Meters per Second.	
	Diameter in millimeters.	Galena, sp gr. 7 5.	Quartz, sp. gr. 2.6.
Group I.	64.0	1.571	0.780
	45.2	1.322	.671
	32.0	1.112	.551
Group II.	22 6	.922	.457
	16.0	.786	.390
	11.3	.661	.328
	8.0	.556	.276
Group III.	5.6	.463	.230
	4.0	.392	.195
	2.8	.327	.161
	2.0	.278	.137
Group IV.	1.4	.232	.115
	1.0	.195	.097
	0.71	.165	.082
	0.50	.138	.073
Group V.	0.35	.116	.058
	0.25	.098	.048
	0.125	.069	.034

Group VI. comprises fine slimes.

factory sieve-scale a list of sieves beginning with one having 1 mm. hole, and ranging upwards in geometric progression by the factor $1.4142 = \sqrt{2}$, and downwards by the reciprocal $\frac{1}{1.4142}$. He proceeds to show (p. 271) that not all the sieves of this scale are needed for separating two minerals of greatly diverse specific gravity; for instance, quartz and galena. This may be deduced from the foregoing table, constructed from his figures.

The smallest grain of galena in any one group will settle more rapidly than the largest grain of quartz in that group.

The sieve-scale required for quartz and galena, as shown by the above table, is then:

Group.	Mesh in millimeters.	
I.	Through 64.0	on 22.6
II.	" 22.6	" 5.6
III.	" 5.6	" 1.4
IV.	" 1.4	" 0.35
V.	" 0.35	" 0.125
VI.	" 0.125	

It is to facilitate the making of such shortened sieve-scales, adapted to special groups of minerals, that the fall-curve diagrams have been constructed. For instance, if it be desired to construct a sieve-scale for the preparation of sands containing quartz (sp. gr. 2.6) and apatite (sp. gr. 3) for jigging, we start with the proviso that in any given group the smallest grain of apatite must have 1 centimeter per second greater velocity than the largest grain of quartz in that group. Taking 1 centimeter = 0.01 meter diameter as the limiting sieve under the rolls, we can then select by inspection from the diagram (Plate II.) the following sieve-scale.

Class.	Mesh in Meters.		Velocity in Meters per Second.	
	Through.	Upon.	Largest Quartz.	Smallest Apatite.
I.	0.01	0.0084	0.304	0.316
II.	0.0084	0.0070	0.280	0.290
III.	0.0070	0.0060	0.256	0.266
IV.	0.0060	0.0050	0 236	0.245
V.	0.0050	0.0042	0.215	0.225

And so on. A similar sieve-scale for classifying, preparatory to jigging, the grains of any two minerals of different specific gravities can be made from the diagrams, with less trouble, we think, than in any other way.

The velocities given in the curves are all of them final velocities, calculated by Rittinger's formula. In quick jigging the conditions of his formula would not be quite realized, as the particles would not have time to acquire their final velocity. To give some idea to what extent the curves would be in error for quick jigging, we add the following table, taken from Rittinger, (p. 178,) but much enlarged.

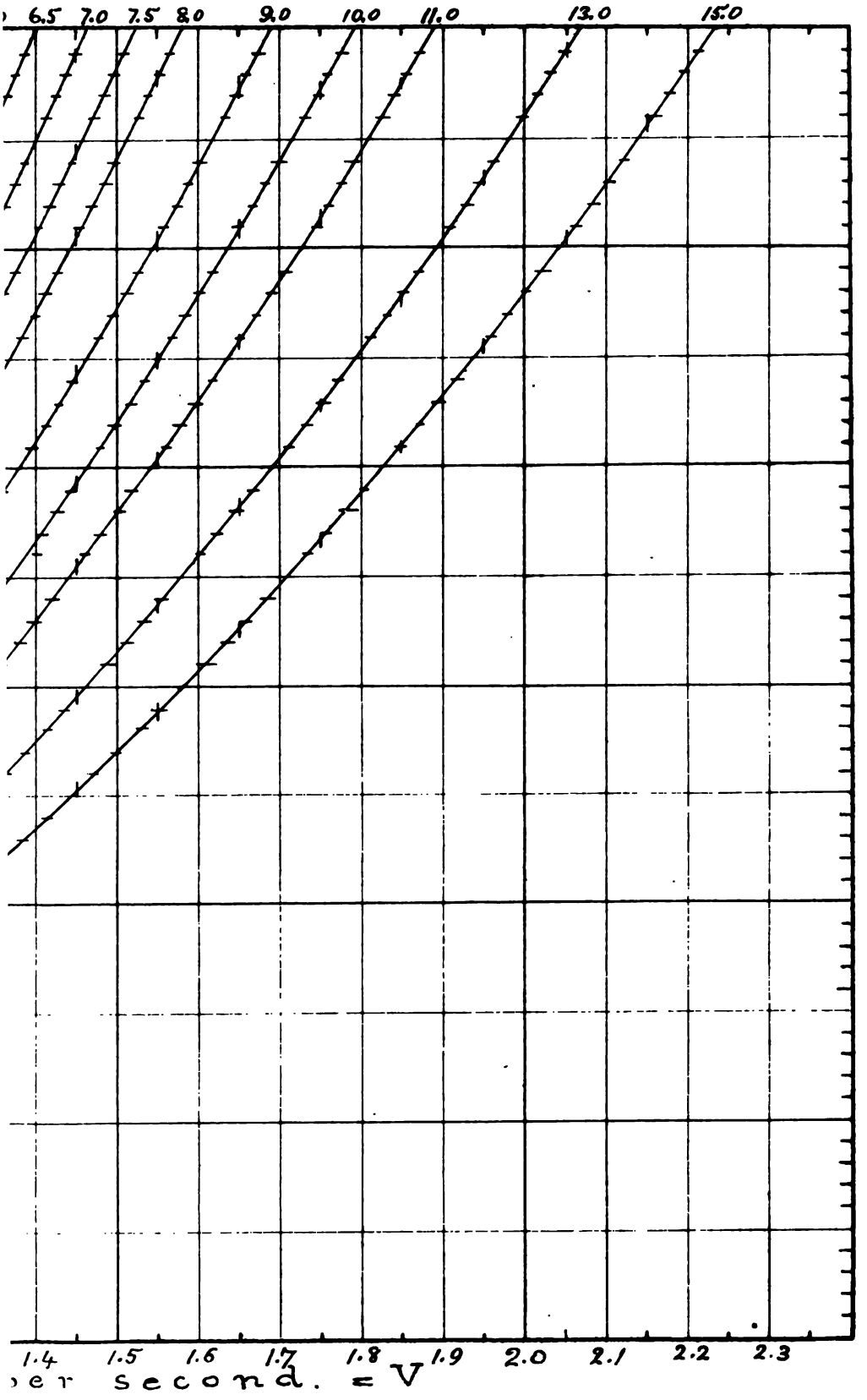
Substance.	Diameter in Meters.	Fall in Meters per Second.					
		$l = 0.062$.	$l = 0.125$.	$l = 0.25$.	$l = 0.50$.	$l = 1.0$.	$l = 2.0$.
PbS	.016	0.514	0.936	*1.416*	*1.630*	*1.650*	*1.650*
FeS_2	.016	0.468	*0.825*	*1.174*	*1.287*	*1.293*	*1.293*
SiO_2	.016	0.353	*0.570*	*0.767*	*0.801*	*0.817*	*0.817*
PbS	.012 ($\frac{1}{2}$ in.)	0.508	0.902	1.290	1.388	1.428	1.428
FeS_2	.012 ($\frac{1}{2}$ in.)	0.461	0.789	1.054	1.118	1.120	1.120
SiO_2	.012 ($\frac{1}{2}$ in.)	0.345	0.558	0.689	0.708	0.709	0.709
PbS	.008	0.497	0.842	1.107	1.164	1.166	1.166
FeS_2	.008	0.448	0.723	0.890	0.914	0.915	0.915
SiO_2	.008	0.332	0.499	0.572	0.578	0.579	0.579
PbS	.006 ($\frac{1}{4}$ in.)	0.487	0.790	0.980	1.009	1.010	1.010
FeS_2	.006 ($\frac{1}{4}$ in.)	0.436	*0.669*	0.778	0.792	0.792	0.792
SiO_2	.006 ($\frac{1}{4}$ in.)	0.319	0.454	0.498	0.501	0.501	0.501
PbS	.004 ($\frac{1}{6}$ in.)	0.468	*0.704*	*0.814*	*0.823*	*0.824*	*0.824*
FeS_2	.004 ($\frac{1}{6}$ in.)	0.414	*0.586*	*0.643*	*0.646*	*0.646*	*0.646*
SiO_2	.004 ($\frac{1}{6}$ in.)	0.297	*0.383*	*0.409*	*0.409*	*0.409*	*0.409*
PbS	.003 ($\frac{1}{8}$ in.)	0.451	0.645	0.710	0.714	0.714	0.714
FeS_2	.003 ($\frac{1}{8}$ in.)	0.394	0.527	0.559	0.560	0.560	0.560
SiO_2	.003 ($\frac{1}{8}$ in.)	0.282	0.344	0.354	0.354	0.354	0.354
PbS	.002 ($\frac{1}{12}$ in.)	0.421	0.553	0.582	0.583	0.583	0.583
FeS_2	.002 ($\frac{1}{12}$ in.)	0.362	0.445	0.457	0.457	0.457	0.457
SiO_2	.002 ($\frac{1}{12}$ in.)	0.250	0.286	0.289	0.289	0.289	0.289
PbS	.001 ($\frac{1}{25}$ in.)	0.354	*0.409*	*0.413*	*0.414*	*0.414*	*0.414*
FeS_2	.001 ($\frac{1}{25}$ in.)	0.294	*0.321*	*0.323*	*0.323*	*0.323*	*0.323*
SiO_2	.001 ($\frac{1}{25}$ in.)	0.195	*0.203*	*0.204*	*0.204*	*0.204*	*0.204*
PbS	.0005 ($\frac{1}{50}$ in.)	0.277	0.291	0.292	0.292	0.292	0.292
FeS_2	.0005 ($\frac{1}{50}$ in.)	0.222	0.229	0.229	0.229	0.229	0.229
SiO_2	.0005 ($\frac{1}{50}$ in.)	0.143	0.145	0.145	0.145	0.145	0.145

The Italic figures were taken from Rittinger, the others were calculated.

$$V = \frac{1}{A} \times \frac{e^{2Bt} - 1}{e^{2Bt} + 1} \text{ when } B = \frac{g(s-\Delta)}{s} A, \text{ and } A = \sqrt{\frac{3 a_3 \Delta}{2 d j (s-\Delta)}}.$$

v = velocity of fall in meters per second.
e = Napierian base.
g = acceleration due to gravity.
s = sp. gr. of solid.
Δ = sp. gr. of liquid.
d = diameter of solid.
j = weight of cu. in. of water.
a_3 = constant velocity v after t in seconds.

All jigging will practically take place between $t = .25$, or 240 strokes per minute, and $t = .50$, or 120 strokes per minute. Now if we follow down these two columns in the above table, we shall see that this error is serious for 240 strokes with $\frac{3}{8}$ inch, $\frac{1}{2}$ inch, and $\frac{1}{3}$ inch diameter; becomes insignificant with $\frac{1}{4}$ inch, $\frac{1}{6}$ inch, and $\frac{1}{8}$ inch, and disappears practically in $\frac{1}{12}$ inch, $\frac{1}{25}$ inch, and $\frac{1}{50}$ inch diameter; that for 120 strokes per minute it is nowhere serious; is insignificant for $\frac{3}{8}$ inch, $\frac{1}{2}$ inch, and $\frac{1}{3}$ inch diameter, and practically disappears for $\frac{1}{4}$ inch and all below.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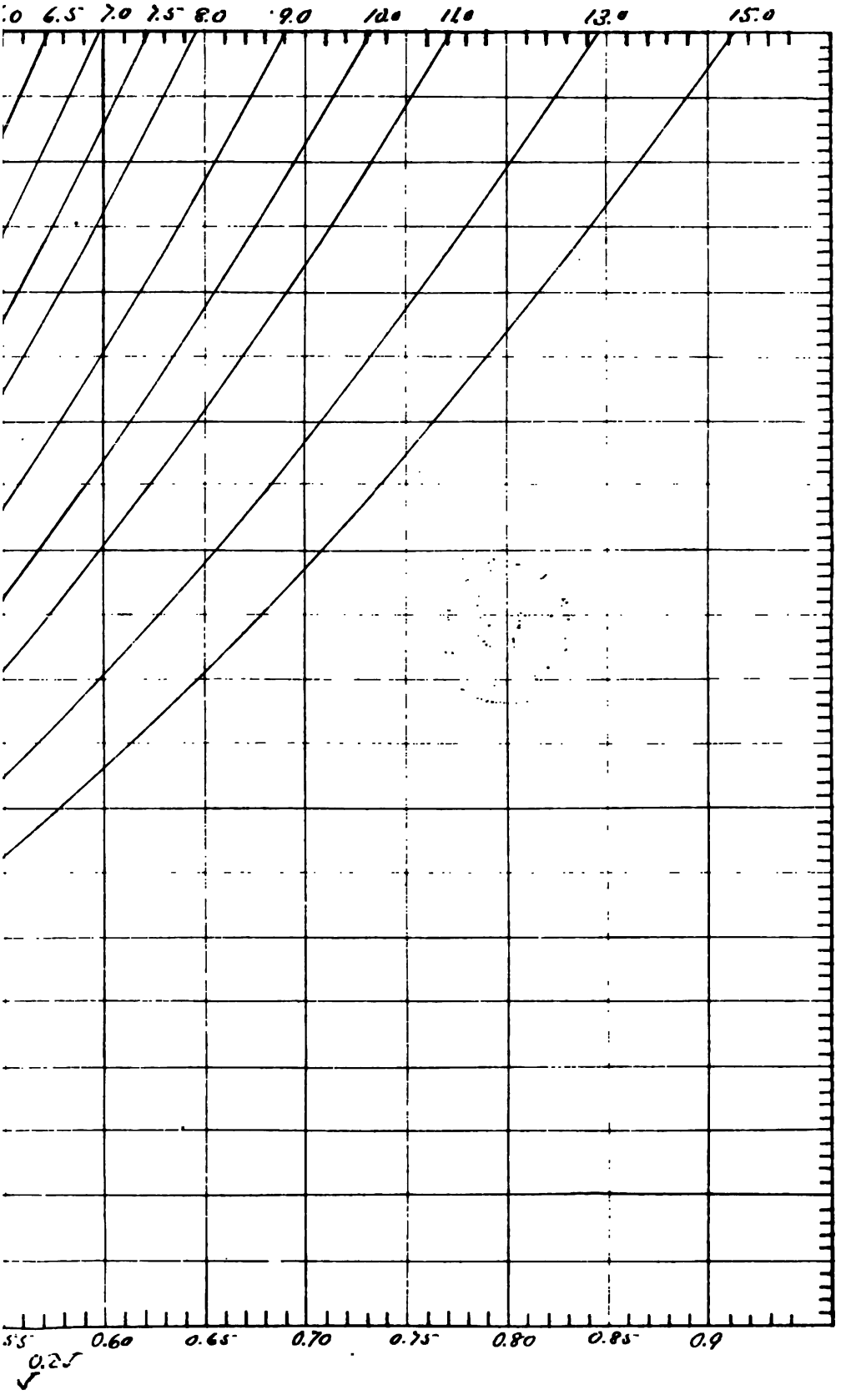
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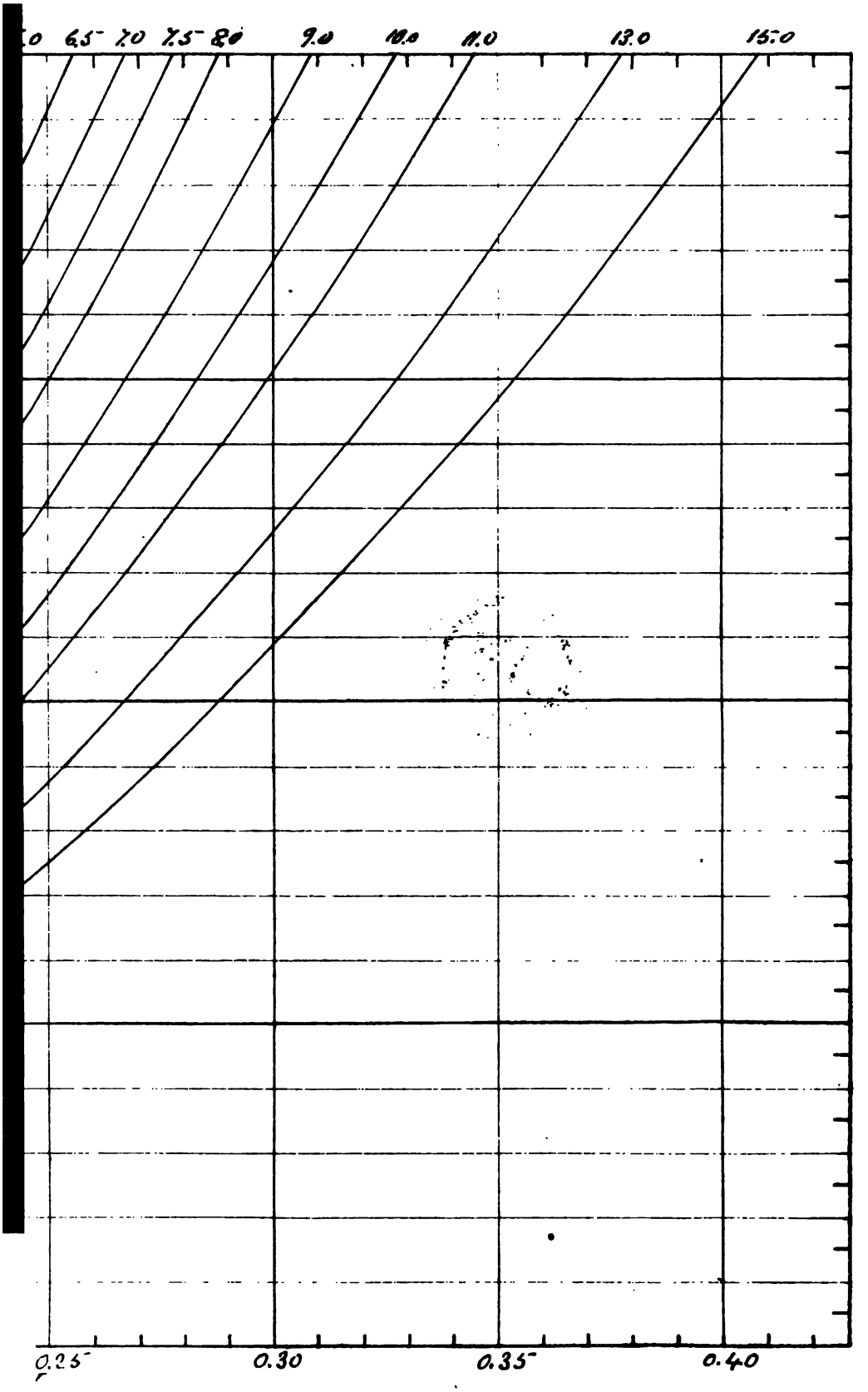
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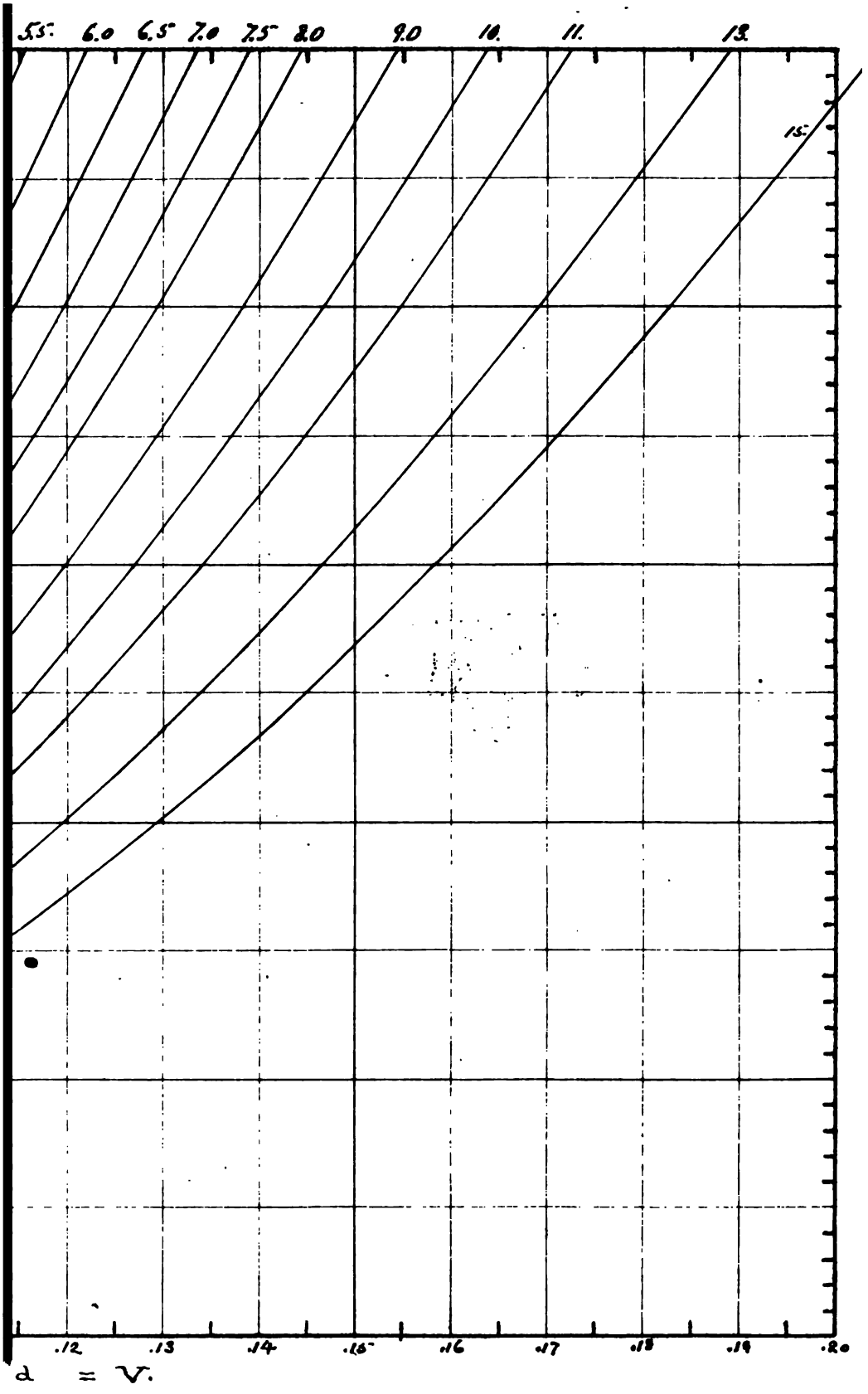
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AN ATTEMPT TO REDUCE ARCHITECTURAL DESIGNING TO A LOGICAL BASIS.

BY C. HOWARD WALKER, INSTRUCTOR IN DESIGN.

THE relation of architecture to the exact sciences is far from being clearly defined. Combining as it necessarily does the elements of a science with those of an art, it has been looked upon askance, on the one hand as indefinite in its methods, on the other as occasionally too mechanical in its results.

As far as construction is concerned, graphical statics and formulæ supply all needs with sufficient exactness; for architecural construction seldom reaches the inspirational stage, and when such an event occurs the assistance of an engineer is usually solicited.

But construction is not, apart from its necessary adequacy, the most important part of architecture, which is to be regarded for its æsthetic qualities. The objection can justly be made, that it is assumed that the inspiration of genius and the vague, intangible æsthetic sense disarm all criticism. Æsthetic, curiously enough, however, is defined as "pertaining to the science of beauty," and science means certain knowledge; therefore it would seem that the principal thing desired in architectural study should be the certain knowledge of beauty.

The completeness of this knowledge is frequently questioned, and it is presumed that there is no fixed standard and no fixed set of laws by which beauty can be determined or gauged; a freedom from bondage, an erratic liberty of expression, being its prerogative by a sort of divine right, and essential to many of its forms. Yet upon observation there seems to be a very close relation between beauty and utility, between the pleasing thing and the necessary thing, and whatever is given to one at the expense of the other appears to injure both.

It is a perfectly easy matter to attempt to beautify at the expense of constructive expression, and just along this line lie most of the errors of æsthetic taste; but it is a very difficult matter to construct well at the expense of artistic expression, since the influence of the force of gravity alone requires an organic method, and a set of forms, masses, and lines, which will be found more pleasurable to the eye in proportion as they more nearly perform their exact duty.

Whether this is from hereditary perception of instability, or from the tendency of like to attract like, — man, a highly developed organism, being appealed to most strongly by the organic quality in his environment, — is a subject for mental philosophy; that the fact exists is undeniable.

It follows, then, that there are two methods of making a building ugly; first, by lack of development of its organic qualities, that is, by crude and inefficient expression of its constructive requirements, and, secondly, by the addition of non-essential forms, so-called artistic accessories, contrary to those requirements.

Conversely, there are two methods of making a building beautiful; by the fullest and most subtle development of constructive requirements, and by the application of forms which accentuate the organic qualities of the work.

Taking this for granted, — and, certainly, but slight objection can be made to the general statement, — the basis of all design must necessarily be a thorough sense of constructive requirements in order to possess the knowledge of the proper points to accentuate; and this should be, not a perception of a number of petty details, of minor threatening weaknesses, but a pervading sense of the great law governing all building, the law of gravitation.

The first necessity, then, is stability; which means that all main lines, except those of metal, must be horizontal or perpendicular, and that the bottom must not be incapable of supporting the top. This latter necessity has a marked influence upon the disposition and the size of openings, and creates the general rule that voids are preferably placed over voids and solids over solids.

The position of the centre of gravity, real or apparent, creates the desire for more solids in proportion to openings at the lower part of the building than at the upper, and the consequent increase in number of openings in the upper part; and the same desire makes the two sides of a building of the same general density, preventing the weighting of one side more than the other.

This wish for apparent balance is the reason for symmetry, which is, practically, corresponding density on either side of a central axis, and which occasions, to a great degree, the planning of buildings on axes, and the subsequent balancing of the masses of the façade. As far as construction is concerned, this need of lateral balance is imaginary; but it is the first and only important departure of the æsthetic requirement from the constructive one, and has as its basis the feeling that all masses are most stable when equally distributed upon a horizontal plane.

Theoretically, this is true, so that the artistic sense, in this case at least, seems to require more rigid adherence to the theoretical law of solids than does the constructive necessity; a rather suggestive fact to those who claim that art is irrational in its methods.

The wall, and columns carrying a lintel or a wall, are the simplest of constructive units, and are the antecedants of all early architectural forms until the arch makes its appearance.

To the wall, as it is built in horizontal courses, is applied the horizontal scheme of lines; to the column and its associate, the pier, the perpendicular scheme. In each the æsthetic method accents the constructional precedent; in one, by the use of accented jointings and by moulded string-courses, cornices, etc.; in the other, by flutings and panels.

The horizontal lines carried through a façade are influenced in their relation to one another in size and distance apart by two desires: first, to make the lower part of a building seem stronger than the upper; secondly, to divide organic portions of the building one from another.

The first influence is felt in the grading of horizontal joints, so that the larger and wider stones are below, and the interval between the joints decreases toward the top, and, as in the use of more numerous openings as the wall ascends, there is produced a graded tone, a tone of comparative strength, expressed by a gradual shading toward the top of the building. The same result is obtained frequently by changing the texture of the stone, as, for instance, when rock or split-face is used in the first story, and cut ashlar above. The American custom of having the first story of buildings of a different and stronger material than is the rest of the façade is a logical result of the same reasoning.

The same diminution of tone appears in the scale of mouldings as they ascend in position. There is, however, a prevalent idea, caused by the fusing of forms by distance, that all ornament near the eye should be smaller than that far from it; but while this may be perfectly true of the finish of detail, the breadth of treatment and interval of repeat and of decorative scheme should be greater at the base of a building than at its top.

The second desire affects the importance of the base, belt, and cornice mouldings.

It is usual, and has become an unchallenged custom, to consider a strong base and a strong cornice as essential features in an architectural design, and constructional derivations are constantly stated

for both; but there seems to be little understanding of the æsthetic reason for the disposition and relative size of bands of mouldings. This reason is, that each organic portion of a building should be defined at its boundaries, and these bands or belts are treated as so many borders confining parts of the building, and separating each part from its neighbor.

The scale of these borders depends upon the quantity of space confined within their parallel lines, and upon the importance of the change from one portion of the building to another; for instance, the base course and the cornice confine between them the entire mass of the façade, and are made, relatively, the strongest borders.

The lower belt courses are apt to confine higher masses of wall than the upper, and are consequently larger.

The entablature has its divisions of epistyle frieze, and cornice, defined by much smaller borders, as those divisions are secondary and of minor importance.

When portions of the same plane are divided to express organic parts, such as the different stories, etc., the belts have comparatively little width or projection; but when change of plane, or change of form of the solid occurs, the mouldings and decoration become at once more important and more insistent in emphasizing the change. For instance, the cap is inserted where the cylinder of the column changes to the plane surface of the epistyle, and the richness of bed-moulding and soffit where the plane of the cornice breaks forward from the plane of the frieze. This is true of all changes from one geometric solid to another, whether in Classic, Gothic, or Mohammedan architecture,— the more fundamental the change, the firmer and richer become the division lines of moulding defining the point of demarcation.

With the advent of the arch appears a new series of subjects for consideration, first of which is the fact that all arch lines must maintain their integrity throughout, and must be unworried by other lines impinging upon them. Secondly, the arch must be sufficiently weighted both at crown and at haunches, the æsthetic desire here, as is frequently found elsewhere, warning of constructive danger before it becomes actual.

The introduction of the curves of arches also brings up the question of the similarity of quality in detail on a façade; that is, that there should be an equality of strength expressed throughout, and not in one place a low flat arch with great thrust and little resistance, and in another a tall pointed arch with wedge-like obstinacy.

If such change of quantity of force were to be made, it should be

disposed in such a way as to insure an equilibrium of forces; the whole façade should seem to be in equipoise, not with one portion weaker than another.

The same is to be said of the relative scale of detail, whether it is in openings, mouldings, or curving.

Wherever the planes of a solid change at a more obtuse angle than 135°, the change needs to be defined by mouldings, etc.

Wherever geometric solids terminate in a point, it should be accented by some sort of finial; in fact, an exaggeration of visually uncertain constructive facts is one of the chief motives of architectural detail. This is demonstrated by the use of vaulting ribs, rosettes at intersections, triglyphs, gutters, crockets, finials, etc.

There is an axiom in attacking architectural design from a logical standpoint, that wherever quantity of material and consequent inertia is sacrificed, it is made up in vigor of constructive expression.

When the sturdy quality of the Doric is supplanted by the lighter and more graceful Ionic, the flutes of the column become deeper, the horizontal lines of the entablature draw closer together and become more frequent. When the vigorous columns of the Romanesque are multiplied, clustered, it is because the openings in the wall are becoming larger; and, at length, when the Gothic becomes an organism of masses in equilibrium, each pier, column, rib, and arch has its purpose written upon it by insistent lines drawn in the direction of its resisting force.

But the inertia of horizontal masses is injured by being shot through with the nervous vigor of perpendicular supports, and the energy of ascending columns and piers is dulled by the heavy long lines of inert masses. Architecture is not a stuff with warp and woof; it needs a dominant motive, for otherwise the æsthetic sense suspects instability. To it exact equilibrium is uncertain equilibrium, and it prefers the certainty of dominant horizontal or perpendicular lines to the plaid of conflicting forces. Above all, a building is an organism of which each part should perform its function, not only actually but manifestly, and architectural design should be devoted to denoting unmistakably how each function is performed.

Disguise is affectation. Frank exposition of construction is alone commendable, giving logical quality to design; and it is soon apparent that the æsthetic desire, once recognizing this fact, will suggest the most satisfactory construction, while at the same time it satisfies itself.

BIOPHENE.

BY LOUIS E. LEVI, PH. D.

THE gap which now separates the fat from the aromatic series is being rapidly filled. The great researches upon pyridine, pyrrol, furfurane, thiophene, and other groups of bodies similarly derived from the fat series, have already shown to chemists that the words "fat" and "aromatic" must soon be changed to some more appropriate terms. All the reactions which the above groups possess, although they are derived from bodies in the fat series, show them to be closely related to the aromatic compounds.

The actions of acids, halogens, and all other reagents, upon these groups, are more marked, and progress more easily and rapidly, than upon the aromatic compounds. The thiophene discovered by Victor Meyer, being the first body containing sulphur and possessing the properties of aromatic bodies, is of especial interest. During the time the writer had the pleasure of working under Victor Meyer, and later of working with him, the idea of a body similar to benzol, in which two "CH" groups are replaced by two sulphur atoms, was first suggested. The interest in the question, whether a body with the symbol

would also possess the properties of an aromatic compound, was very great. Since the action of phosphorus trisulphide upon succinic acid, $COOH-CH_2-CH_2-COOH$, forms thiophene,

so thiodiglycollic acid, $COOH-CH_2-S-CH_2-COOH$, should, under the same conditions, produce the body which we will call "biophene": —

The probability of the formation of biophene by this reaction is increased by the fact, that while succinic acid and phosphorus trisulphide yield thiophene, thiodiglycollic acid is composed of two half-molecules of succinic acid joined together by an atom of sulphur. Biophene is produced, as was expected, by the above reaction, but only under certain conditions.

The method of distilling the acid with the phosphorus trisulphide, the way in which thiophene is made, yields an impure product containing very little biophene, but yet sufficient to show the characteristic indophenin reaction. In order to obtain biophene in sufficient quantity and purity, the method of its preparation was modified.

Preparation of Biophene.

Five grams of thiodiglycollic acid are mixed with ten grams of phosphorus trisulphide, and placed in a tube, closed at one end, with fifteen to twenty cubic centimeters of ether. The tube is then closed with a fine capillary, placed in an oven, heated two hours at a temperature of one hundred and seventy degrees centigrade. After cooling, the capillary is opened to allow the escape of the hydrogen sulphide formed. The contents of the tube, which has an odor not unlike the secretions of a skunk, is placed in a separating funnel, and washed with potassium hydrate. The alkali is then drawn off, and the remaining oil dissolved in ether and dried with chloride of calcium. After drying thoroughly, the ether is evaporated and the substance fractionated. The oil passes over in fractions between 140° and 170° C., accompanied by a slight decomposition. By refractionating, an oil having the constant boiling point 165°–170° is obtained. This product, when mixed with sulphuric acid and a crystal of isatine added, gives a beautiful violet color, the characteristic indophenin reaction of the thiophenes. The analysis showed that this oil was biophene.

0.180 gr. of the oil oxidized with HNO_3 yielded 0.7208 gr. $BaSO_4$.

	Found	Theory for $C_4H_4S_2$.
Sulphur	55.00	55.17

ACETO–BIENONE, $C_4H_3S_2COCH_3$.

To a mixture of one gram of biophene, ten grams of petroleum ether, and one gram of acetyl chloride, aluminium chloride is slowly added. A reaction begins immediately, accompanied by the evolution of hydrochloric acid gas. If the flask is shaken every five minutes, the reaction will be quickly finished, and the aluminium compound of the ketone will settle to the bottom of the flask. When on further addition of aluminium chloride no more hydrochloric acid gas is given off, the product is warmed about fifteen minutes upon the water bath, and the petroleum ether poured off. The thick oily mass is then decomposed with ice and water, and purified by distilling with steam. The distillate is extracted with ether, and dried over chloride of calcium. On evaporating the ether, there remains a heavy, slightly colored oil, whose odor is very aromatic and like that of aceto-thienone.

Aceto-bienone distils at a temperature above 300° C., at the same time decomposing. The action of light upon it turns it dark brown. In order to characterize aceto-bienone as a ketone, the phenylhydrazine compound was made.

ACETO–BIENONE AND PHENYLHYDRAZINE.

Molecular weights of aceto-bienone, phenylhydrazine, and anhydrous sodium acetate are heated upon the water bath, with a little water, about six hours. On cooling, beautiful red-colored needles precipitate, which upon recrystallizing out of alcohol are obtained pure. The phenylhydrazide has a melting point of 128°, and is soluble in alcohol and ether.

0.156 gr. of the salt oxidized with HNO_3 yielded 0.2905 $BaSO_4$.

	Found.	Theory for $C_4H_3S_2C\overset{CH_3}{\underset{N_2HC_6H_5}{\diagdown\!\!\diagup}}$
Sulphur	25.6	25.8

OXIDATION OF ACETO–BIENONE.

To one gram of aceto-bienone take 2.55 gr. $KMnO_4$, and add fifteen cubic centimeters of a 14% KOH solution for every 50 c. c. of a 2% $KMnO_4$ solution. This mixture is placed in a flask with a return cooler attached, and agitated continually. The reaction begins immediately, and the liquid assumes a dark green color. As soon as the green color disappears, the substance is heated on the water bath for thirty minutes, when the oxide of manganese which is formed will settle to the

bottom, leaving a clear liquid. The oxide of manganese is removed by filtration, and the liquid acidified with sulphuric acid and extracted with ether. After the ether solution is evaporated an oil remains, which, on standing, crystallizes. The crystals are purified by crystallization from alcohol or water. The water solution shows the characteristic acid tests with litmus paper and sodium carbonate. The yield being small, an analysis could not be made.

PHENYLBIENYLKETONE, $\begin{matrix} C_6H_5 \\ C_4H_3S_2 \end{matrix}\!\!\rangle CO$,

is formed by the action of benzoyl chloride upon biophene in the presence of aluminium chloride in the same way as the above described aceto-bienone. Phenylbienylketone is a dark brown oil, having a boiling point of 241° C., and is soluble in alcohol and ether.

0.2933 gr. oil yielded 0.6310 gr. $BaSO_4$.

	Found	Theory for $\begin{matrix} C_6H_5 \\ C_4H_3S_2 \end{matrix}\!\!\rangle CO$.
Sulphur	29.18	29.09

NITROPHENYLBIENYLKETONE.

Two grams of phenylbienylketone are slowly mixed, drop by drop, with nitric acid (sp. gr. 1.62), cooled by a freezing mixture of Glauber's salt and hydrochloric acid. As soon as the two substances come in contact, a violent reaction takes place, and the oil is completely dissolved. On cooling, the product of the reaction is diluted with water, and a light yellow mass precipitates. The precipitate is then placed upon a filter, and washed with water until an acid reaction is no longer noticeable. The dried substance is then crystallized from acetic acid, and shows a melting point of 112° C. Alcohol and ether can be used as crystallizing agents, but are not so good, as the substance crystallized from them will still have a slight yellow color. One would expect, from the analogy between this substance and the aromatic bodies, that a dinitro compound would be formed, yet such is not the case. The mononitrophenylbienylketone, in long, needle-like crystals, is formed. The analysis of the substance is as follows: —

0.0893 gr. of the salt yielded 4.45 c. c. nitrogen, at a temperature of 21° C. and a pressure of 745 mm.

	Found.	Theory for $C_{11}H_7S_2O_3N$.
Nitrogen	5.55	5.29

The results of this work show once more that compounds may be formed from the fat series which show decided characteristics of the aromatic series, and which may be regarded as belonging to it; also, the similarity between biophene and thiophene and benzol has been shown. In conclusion, let me add that the action of phosphorus trisulphide upon thiodilactylic acid has been tried, and a product obtained which shows the isatine reaction, proving that in all probability the dimethylbiophene or bioxene can also be obtained in the same manner as biophene.

MASS. INSTITUTE OF TECHNOLOGY,
April, 1890.

HARCOURT'S PENTANE STANDARD LAMP.

BY H. E. CLIFFORD, INSTRUCTOR IN PHYSICS.

THE great progress which has been made of late years in artificial lighting, particularly the enormous development in lighting by electricity, has given to photometric tests of the different illuminants a considerable importance. In making such tests, we are at once met by the difficulty of securing a satisfactory standard of light for reference. The so-called standard candle, which, for lack of something better, is at present almost universally adopted as a legal standard, is far from constant in its illuminating power, being affected by slight variations in the material and form of the wick, temperature of the photometer room, and numerous other elements.

It seems desirable, however, in the selection of some new standard, that it should be approximately equal to the present standard candle; the expression *so many candle-power* conveying a certain idea, although in most instances a vague one, as to the amount of light which is given by any illuminating source. Constancy of composition in material consumed, effect of temperature and pressure of air upon the intensity of the light, and ease of manipulation, are also to be kept in view in the selection of a legal standard.

The following tests of the Harcourt Pentane Standard Lamp were carried on in April and May, 1888, by Messrs. E. R. Pearson and S. C. Hathaway, Jr., at that time students in electrical engineering at the Institute of Technology. The pentane was prepared by Mr. Pearson from what is known as No. 86 gasolene, and after being refined was of specific gravity 0.6319. The lamps used in the tests were two, one lent by Mr. C. F. Prichard, Superintendent of the Lynn Gas Light Company, the other the property of the Institute. The Pentane Lamp was devised by Mr. Harcourt to replace the Harcourt Air-gas Standard, which is not portable, and, moreover, involves the use of expensive apparatus. The nature of the flame is the same as in the Standard, the essential difference between the two units being that in the Standard the gas is prepared before its application, while in the Lamp it is produced at the instant required. The gas issues from a circular aperture one fourth of an inch in diameter, cut in a brass plug one inch

in diameter and half an inch thick, the height of the flame and number of drops of liquid pentane consumed per minute being easily regulated. In all tests the lamps were allowed to burn either fifteen or thirty minutes before beginning the photometer readings, so that they might become of a constant temperature. The tests were of fifty readings each, unless otherwise specified. In determining the illuminating power in terms of candles, standard sperm candles were used, corrected to a rate of consumption of 120 grains per hour. The lamps were tested against each other, the Lynn lamp being taken as the unit, and were reversed in position. The Bunsen photometer was used in the tests, the photometer room being painted a dull black, and carefully screened to prevent introduction of error by reflection. The photometer bar was 100 inches in length.

In Tables I. and II. are given summaries of the tests of the two lamps against each other, the lamps for Table II. being reversed in position. In Tables III. and IV. the results of comparing the lamps with standard candles and with the Methven Long and Short Slot are given, the Methven being expressed in terms of the Harcourt Lamp. The illuminating power is the mean as determined from fifty observations, the deviation is the average deviation from the mean, and the percentage deviation is given with regard to the mean illuminating power. The difference in illuminating power, as shown on reversal

TABLE I.

HARCOURT *vs.* HARCOURT.

No. of Observations.	Illuminating Power.	Deviation.	Per cent Deviation.
25	1.021	0.014	1.37
25	1.017	0.016	1.57
50	1.008	0.009	0.89
50	1.014	0.011	1.08
50	1.016	0.008	0.78
50	1.034	0.008	0.74
50	1.028	0.010	0.99
50	1.011	0.012	1.16
50	1.018	0.009	0.87

TABLE II.

SAME. — LAMPS REVERSED.

No. of Observations.	Illuminating Power.	Deviation.	Per cent Deviation.
50	1.051	0.012	1.01
50	1.056	0.007	0.61
50	1.053	0.008	0.73
50	1.055	0.007	0.70
50	1.055	0.005	0.51
50	1.055	0.006	0.54
50	1.057	0.005	0.51
50	1.050	0.006	0.61
50	1.056	0.005	0.48

TABLE III. — INSTITUTE HARCOURT.

Methven Long Slot.			Methven Short Slot.		
Illuminating Power.	Deviation.	Per cent Deviation.	Illuminating Power.	Deviation.	Per cent Deviation.
1.836	0.009	0.51	1 907	0.011	0.55
1.879	0.011	0 57	1.942	0.011	0.56

TABLE IV. — LYNN HARCOURT.

Candle A.			Candle B.			Methven Long Slot.			Methven Short Slot.		
Illum. Power.	Deviation.	Per cent Deviat.	Illum. Power.	Deviation.	Per cent Deviat.	Illum. Power.	Deviation.	Per cent Deviat.	Illum. Power.	Deviation.	Per cent Deviat.
1.081	0.030	2.77	1.083	0.012	1.06	2.121	0.016	0.78	2.023	0.010	0.48
1.111	0.016	1.41	1.108	0.012	1.07	2.151	0.019	0.86	2.008	0.020	0.99
1.085	0.014	1.24	1.074	0.011	0.97						
1.015	0.012	1.11	1.054	0.015	1.38						

of the position of the lamps, has not been satisfactorily accounted for.

The Harcourt Pentane Lamp, as a legal standard, would seem to be a satisfactory one so far as mere intensity and constancy of illumination are concerned, although the uncertain quality of pentane and the care required in using the lamp are undoubtedly objectionable.

ROGERS LABORATORY OF PHYSICS,
May, 1890.

RECENT THEORIES ON THE FUNCTION OF THE WHITE BLOOD-CELL.

BY EDWIN O. JORDAN, S. B.

EVER since the discoveries of the celebrated Dutch naturalist, Leeuwenhoek (1632–1723), physiologists have known that ordinary blood is not a mere liquid, as it seems to be, but that in the liquid there are many fairly solid bodies called cells or corpuscles. These are of two principal kinds: the red blood-cells, or hæmacytes; and the white blood-cells, or leucocytes. The red are the more numerous, and give the familiar color to vertebrate blood. In every teaspoonful of human blood there are about 15,000,000,000 red corpuscles, but only 30,000,000 white ones. The blood of clams, lobsters, and nearly all other invertebrates, contains no red cells, but only the white.

The cells are the more important living part of the blood, while the liquid in which they float plays chiefly a mechanical part.

The function of the red cells has for a long time been clearly understood. They are carriers of oxygen from the air to the tissues. These cells owe their color to a peculiar substance called hæmoglobin, which is able to enter very readily into chemical combination with the oxygen of the air met with in the lungs, and to surrender the oxygen quite as readily to the more needy tissues of the body at large. The red corpuscles thus become the middlemen for conveying oxygen from the external air to every inmost nook and corner of the body. The difference in color between arterial and venous blood is dependent upon the more or less thorough oxygenation of the hæmoglobin; the arterial being richly, and the venous poorly oxygenated.

While the office of the hæmacyte has been thus clearly made out, that of the leucocyte has until very recently been exceedingly obscure.

In behavior and in form the leucocyte, or white blood-cell, closely resembles an amœba, — a simple, naked cell of mere protoplasm, a tiny lump of living substance. Like the amœba, it is able to effect changes in its position and its form; like the amœba, it has the power of flowing around and finally engulfing in its substance bodies with which it may come in contact; furthermore, it is able to tear to pieces, dissolve, or *digest* bits of organic material which it has thus surrounded,

this process being known as "intracellular" digestion. The amœba is the lowest animal of which we have any knowledge. It is an "undifferentiated" organism, in the relative sense in which that phrase can be used, remembering that what is called undifferentiated or unspecialized is so only in comparison with higher forms, and that the amœba is enormously complex as compared with simple aggregations of non-living matter.

The leucocyte likewise is an undifferentiated cell; it is the most unspecialized of any cell in the human body, and its structure and appearance at once remind one of the amœba. It is a natural conclusion that the leucocyte must have inherited, along with its primitive structure, the simple physiological peculiarities of its lowly ancestor. Moreover, the leucocytes have very much the appearance and properties of the cells forming the very young body before the different tissues have become recognizable. They alone retain unchanged the original ancestral form; they are cells left over, so to speak, in the process of body-making. They unite in one individual those fundamental properties of contractility, irritability, and secretion, each of which in the adult animal has been taken up and exalted by particular cells at the expense of other functions. The muscle cells are pre-eminently contractile, the nerve-cells pre-eminently irritable, while the cells of the salivary glands, the liver, and the pancreas devote their best energies to secreting and storing up material for future use. To the leucocyte alone does it seem difficult to attribute any one function *par excellence*. Its protoplasm is undifferentiated, crude, raw material as compared with the highly elaborated product in brain, muscle, and gland cells.

What, then, is the function of the white blood-cells? Bearing in mind their simple unspecialized structure, various answers have been given. It has been said, that, "being undifferentiated and specialized to no line of work, they are ready to take up any that comes to hand, and may be compared to the young men in a community who have not yet selected an occupation and are on the lookout for an opening." This view, however, can no longer be strictly and logically held, for it is highly improbable that the leucocytes in an adult animal turn now into muscle cells and now into nerve cells, as some of the young men seeking an occupation choose to be carpenters, and others physicians and lawyers. Indeed, such a view is distinctly contradicted by the history of the different tissues. In the same line of thought is the suggestion — it is hardly more — that since some white blood corpuscles develop into red ones, this may be regarded as their special function.

For the last few years, however, attention has been directed more and more sharply upon a particular attribute of the leucocyte. The leucocyte's power of engulfing various substances has already been alluded to. This must be further emphasized. The leucocytes in a drop of blood will move toward yeast cells, or carmine granules, or indeed almost any solid particles which may be supplied to them, and can be seen with the microscope to surround and envelop the foreign bodies precisely as an amœba would do. As De Bary expresses it: "If a foreign body comes into contact with the surface of the amœboid cell, the latter puts out processes which embrace it and gradually close over it, as the waves close over a drowning animal, so that it lies at last inside the soft cell substance. It may be cast out again at some future time, but it may also suffer decomposition inside the cell, be killed, and disappear."

The possession of this capacity of intracellular digestion has brought the leucocyte into great prominence in several lines of work, one being *the problem of the absorption of fat into the body.* To physiologists it is a well known fact, that of the three great classes of food stuffs, the albuminoids, the starches, and the fats, the entrance of the fats into the body has been most difficult to explain. For while the albuminoids and the starches are soluble in the fluids of the alimentary canal, the fats persist in the form of finely divided particles; so that, while the first two groups of foods may pass through the membrane of the alimentary tract by diffusion, aided perhaps by the suction-pump action of the villi, the fats must pass through as small solid bits.

It is still a vexed question as to just how the passage of the fat is accomplished. Fat globules are found in the alimentary canal; they are also found in the lymph vessels inside the villi. How is the transition effected from one to the other, through a layer of epithelium cells and another layer of connective tissue? It has been supposed by some that the fat passes directly through the columnar epithelium cells lining the walls, and then traverses a system of small canals communicating with one another, and finally with the lymph vessels in the interior of the villus. That is to say, the epithelium cells lining the walls of the intestine grasp the particles of fat and pass them along through the cell substance into the system of canals. On the other hand, it has been maintained by Watney,[1] that the fat does not pass through the substance of the epithelium cells, but between the cells. Other observers hold that both ways of ingress are used.

About 1883, Zawarykin[2] and Schæfer,[3] working independently, came to the conclusion that the fat enters the lymph vessel by no system of

canals, but that the leucocytes bodily transport the particles of fat from the intestine to the lymph cavity of the villus. The leucocytes were actually seen, loaded down with what were apparently fat globules, making their way speedily toward the lymph vessels. Even while admitting that the epithelium cells might play some part in the process, Schæfer yet holds that the leucocytes are the chief means of transportation.

What could be clearer? Fat globules are found in the alimentary canal; the leucocytes, which always swarm in the neighboring tissues after a meal, are also seen to be filled with fat globules; and the same leucocytes, abounding in particles of fat, are found all the way from the canal to the lymph vessel in the villus. Apparently nothing could be simpler. The leucocytes act as beasts of burden, and carry the insoluble fat bit by bit into the body. In so doing, they do what no other cells can do as well. This is their function *par excellence*. As the red blood-cell is the carrier of the food-stuff oxygen, so the white blood-cell is the carrier of the food-stuff fat.

Unfortunately, however, for this interesting and symmetrical hypothesis, there are facts which at present do not allow it to be seriously entertained. One weighty objection is, that the whole matter is based on a case of mistaken identity, and that the fat globules seen in the leucocytes are not fat globules at all. A recent article by Heidenhain[4] effectually disposes of the belief that the globules seen in the leucocytes are fat. Not only are these so-called fat globules susceptible to stain by the aniline dyes, but they persistently refuse to dissolve in ether or xylol. The blunder would seem to have come about through too great reliance on traditional staining methods. Heidenhain gravely says, "As all is not gold that glitters, so all is not fat that stains black with osmic acid."

If Heidenhain's results are confirmed, the main prop of the theory is removed, and the white blood-cell cannot reasonably be considered as the agent by which fat is taken inside the body.

Although on examination of the evidence, *pro* and *con*, the office of fat-carrier must for the present be denied to the leucocyte, there is another and even wider field in which the leucocyte has come to the front. Here, too, the case rests altogether upon the enveloping and digesting qualities of the cell.

The ingenuity of physicians has long been exercised to explain satisfactorily the phenomena of inflammation. The most that can be said is that inflammation is a disturbance of the normal relations between the blood and the tissues. Some of the prominent signs of this disturb-

ance, however, have been exhaustively studied. The pain, caused by tension and pressure upon the sensory nerves; the swelling, redness, and heat brought about by the increased flow of blood to the affected part; the formation of pus, a substance mainly composed of the dead bodies of white blood corpuscles, — are all well known accompaniments of inflammation. When inflammation is produced in the web of a frog's foot, the whole process can be studied in detail with the microscope. In the normal circulation a few white blood corpuscles can be seen sticking to the sides of the small arteries, and passing slowly along by jerks toward the capillaries; but now they collect more thickly upon the walls of the blood-vessels and become more numerous in the capillaries. There is a gathering of the clans of the leucocytes in the neighborhood of the part threatened with inflammation. Very soon they force their way through the cells lining the blood-vessels, and escape into the surrounding tissues, justifying their German name of "wandering cells." The tissue around the veins and capillaries soon becomes crowded with the white corpuscles which have left their normal channels and invaded the inflamed district. These migrating hordes of wandering cells often die in large numbers, and are cast out as *pus*.

The question at once arises, What is the reason for this peculiar migration of the leucocytes? Do they by their death cause inflammation, or do they appear on the scene when mischief is being done by other substances? Are they troublesome seceders, or patriotic defenders?

To answer these questions satisfactorily, we must go back to some famous observations on the behavior of the leucocytes. The occasions upon which the leucocytes display most vigorously their devouring propensities have been studied very thoroughly by Metchnikoff.[5] To Metchnikoff, indeed, belongs the chief place in the development of our knowledge of the leucocytes. He was the first to bring out the suggestive fact, that during the absorption of the tail of a tadpole, amœboid cells are always present in great abundance. These amœboid cells contain scraps of nerve fibre and bits of muscle tissue, and are seen to be making away with the useless tail structure as fast as possible. The fragments enclosed by the leucocytes retain their form for some time, but gradually break up into small globules, and at last merge into the homogeneous cell substance. The same thing is seen during the atrophy of the tadpole's gills. Frog larvæ, moreover, are not the only organisms in which the leucocytes are thus active, for Metchnikoff has described the same process as occurring in the larvæ

of echinodermata, and has shown that the useless organs at each metamorphosis are eaten up by the ever-ready leucocytes. From their possession of this capacity for eating, the leucocytes are often called *phagocytes*, or eating cells. At any inflamed or injured spot, the amœboid cells collect and greedily devour the particles of dead tissue. Whenever and wherever any of the cells of the body die, the phagocytes hasten to remove the corpses. This point is practically certain, that the phagocytes are the scavengers of the organism, the removers of waste and useless material from the body. Have they also a still higher function? Are they the defenders of the organism?

It has long been known that, when a foreign body is introduced into a living blood-vessel, one of the first events is the collection of leucocytes around it. A needle thrust through the wall of a blood-vessel will bring the leucocytes clustering thickly around its point. There is apparently a swarming of the white blood-cells to oppose the invading substance. There seems something akin to this in the case of inflammation also, something more than a mere hastening to remove worn-out tissue, — almost an actual resistance to a hostile influence. The leucocytes appear to resent the presence of anything which can cause harm, and to bend their energies towards removing the offending body; attempting, as it were, to protect the body of the individual as militia do the body politic. Metchnikoff even goes so far as to say, that the process of inflammation is a struggle between irritant bodies and white blood corpuscles.

The relation between the phenomena of inflammation and the invasion of the organism by living parasites is well known. In a paper now become classic, Metchnikoff describes a singular disease that sometimes afflicts the Daphnia, or water-flea. Some of these little creatures, which he kept in a tank in his room for purposes of study, became covered with spores, or conidia, which gained an entrance to the body of the crustacean, multiplied, and were carried by the blood current all over the body. The leucocytes in the mean time do not remain idle spectators of this hostile inroad, but speedily attack and devour the invading spores. If one of the conidia prove too much for a single leucocyte to handle, others join in the assault, and fuse into an invincible "giant cell." The conflict is to the bitter end. "If the leucocytes finally overpower the spores, the Daphnia lives; if not, the conidia completely overrun the crustacean, and death is the result."

According to Metchnikoff, exactly the same struggle takes place whenever an animal body is invaded by bacteria of any sort, as, for example, by the bacilli of anthrax, or splenic fever. In animals such

as frogs and lizards, which are not "susceptible" to anthrax, anthrax bacilli if introduced into the blood are devoured by the phagocytes. The same thing happens when susceptible animals are inoculated with anthrax bacilli which have been "attenuated" or weakened to a harmless condition. But when virulent anthrax bacilli are introduced into the blood of an animal susceptible to the disease, it is only in exceptional cases that the bacteria are taken up by the leucocytes. Metchnikoff therefore asserts, that the anthrax bacillus can cause no harm if it is absorbed and destroyed by the white blood-cells, but that it is dangerous if this does not happen. In other words, the reason that the anthrax bacillus does not do harm is because the white blood-cell envelops and digests it. If for any cause the white blood-cell is unable to perform this duty, either through the high virulence — whatever that may mean — of the bacilli, or through its own incompetence, the bacilli may effect serious and even fatal ravages. This is the essence of the phagocyte theory as advocated by Metchnikoff. When pathogenic bacteria are introduced into the blood of an animal, there ensues a struggle to the death between the bacteria and the leucocytes. If the leucocytes succeed in overpowering the bacteria, the animal recovers; if they fail in this, the bacteria thrive and multiply, and the animal finally dies.

Stated in its most metaphorical form: "If we summarize the story, it should be likened to a battle. The leucocytes are the defending army, their roads and lines of communication the blood-vessels. Every composite organism maintains a certain proportion of leucocytes as representing its standing army. When the body is invaded by bacilli, bacteria, micrococci, chemical or other irritants, information of the aggression is telegraphed by means of the vaso-motor nerves [*sic!*], and leucocytes rush to the attack; reinforcements and recruits are quickly formed to increase the standing army, sometimes twenty, thirty, or forty times the normal standard. In the conflict, cells die, and are often eaten by their companions; frequently the slaughter is so great that the tissue becomes burdened by the dead bodies of the soldiers in the form of pus, the activity of the cell being testified by the fact that its protoplasm often contains bacilli, etc., in various stages of destruction. These dead cells, like the corpses of soldiers who fall in battle, later become hurtful to the organism they in their lifetime were anxious to protect from harm; for they are fertile sources of septicæmia and pyæmia, — the pestilence and scourge so much dreaded by operative surgeons." * Though perhaps rather bellicose, the figure

* J. B. Sutton, An Introduction to General Pathology, p. 127.

just drawn is not unnatural. The phagocyte theory is almost captivating in its dramatic simplicity.

There are, however, many biologists who do not agree with Metchnikoff's interpretation of his observations, although accepting the observations themselves as undoubtedly correct. As to the facts, there can no longer be any question. It is as certain as any such thing can be, that bacteria of various kinds have been seen engulfed in the substance of the white blood corpuscles. Metchnikoff's experiments have been repeated and his statements confirmed time and again by himself, by Wyssokowitsch,[6] Hess,[7] Nuttall,[8] and other competent observers.

The point at issue is simply this: Do the leucocytes march upon and devour the living bacteria? or do the bacteria die in the blood because they are in other respects in unfavorable circumstances, and then, when the bacteria are already dead, do the leucocytes step in and devour the corpses? Are the leucocytes brave soldiers, or only faithful scavengers? The mere fact that bacteria have been seen inside a white blood-cell of course proves nothing. Metchnikoff has sketched from observation white blood corpuscles in the act of enveloping anthrax bacilli, and other corpuscles containing anthrax bacilli in various stages of decomposition. Yet clearly in these cases it cannot be determined whether the leucocytes attacked the bacteria while alive, or simply engulfed them when dead.

There are, nevertheless, some considerations which appear to favor the idea that the leucocyte is a fighting cell. It is well known, for instance, that at ordinary temperatures frogs are not susceptible to anthrax, and, though inoculated with virulent bacilli, remain perfectly healthy. If, however, the frogs are kept at about 85° F., the leucocytes in the frogs' blood no longer take up the bacilli as before, and the animals soon become infected with the disease. Metchnikoff explains the fact that the anthrax bacilli are more potent at the higher temperature by the hypothesis that they have been accustomed to deal with and overcome the leucocytes of warm-blooded animals, and therefore are at a disadvantage in combating the leucocytes of the cold-blooded frog. His opponents quickly dispose of this explanation by retorting that the anthrax bacilli are never able to thrive at a low temperature, but soon die in the cold blood of a frog, and are then consumed by the phagocytes. At a higher temperature, the anthrax bacilli are able to exercise their full virulence, and the leucocytes, although also undoubtedly quickened by the warmth, are not able to cope with the active living bacteria, and the latter win a signal victory.

Other facts brought forward in favor of the theory are, that, if an animal be inoculated with a small quantity of bacteria, there will afterwards be found in the blood a much larger number of leucocytes than at first, seeming to show a strengthening of forces to meet the attack of the enemy. It has also been stated, that anthrax bacilli which have been killed by boiling are not so quickly taken up by the leucocytes as are those injected alive, — a fact that would of course indicate that the leucocytes do not trouble themselves about dead, and consequently innocuous bacteria. This would seem materially to strengthen the cause of Metchnikoff and his followers.

On the other hand, it has never been proved experimentally that bacteria have been destroyed by phagocytes, and by phagocytes alone. Baumgarten,[9] Bitter,[10] and others, have urged this utter lack of positive evidence, and have demonstrated how insecure is the basis of the phagocyte theory, judged on this point alone. There is, in addition, a certain kind of evidence of the existence of hostile influences in the body outside of the leucocytes. A number of observations have shown that anthrax bacilli have often suffered some damage before they became the prey of the phagocytes. Some very suggestive experiments by Christmas-Dirkinck-Holmfeld[11] showed that only comparatively few bacteria were taken up by the leucocytes, and that by far the greater number of bacteria were destroyed outside the cell. He proved conclusively that, even if the phagocytes did play some part in the struggle of the organism against harmful foreign bodies, they were not the sole defenders, — theirs indeed was not the chief *rôle*. There are manifestly other influences in the body hostile to bacteria.

This bacteria-destroying influence has been thought to be resident in the blood, the belief being based on the researches of Nuttall,[8] Buchner,[12] Nissen,[13] and others. These observers proved that blood taken from the body possessed germicidal properties, and that blood serum, freed from all corpuscles, could destroy even virulent anthrax bacteria. This remarkable property is lost on heating the serum to 55° C., but at ordinary temperatures persists for days. This fact was thought to give the death-blow to the phagocyte theory, because if blood serum *per se* was able to destroy bacteria, there was clearly no need for supposing that the leucocytes voluntarily attacked living bacteria. It might be held more reasonably that the leucocytes simply took up the bacteria which were already killed or weakened by the influences shown to exist in blood serum.

Very recently an exceedingly important paper by Lubarsch[14] has appeared, in which the author urges that, although the blood *outside*

the body is able to destroy bacteria, this has not been proved true of the same blood *inside* the body. Indeed, his experiments show very plainly that a great difference exists. In one experiment, for example, a single drop of rabbit's blood, when removed from the body, destroyed 53,700 bacilli, but all the blood of the same animal when inside the body could not kill 16,430 bacteria with which the animal was inoculated; on the contrary, the bacteria multiplied, and the animal died. Lubarsch attributes this startling difference to the different conditions of nutrition existing inside and outside the animal body. However this may be, there seems no reason for doubting the fact, that in the living animal the substances normally present in the blood exert no perceptible injurious effect upon living bacteria.

Lubarsch also records a number of experiments bearing more closely upon the phagocyte theory, and concludes from their results, that, although leucocytes do occasionally take up living bacteria, there is no such wholesale destruction of the bacteria by the white blood corpuscles as supposed by Metchnikoff's theory. He recognizes rather an influence proceeding *directly from the body cells*, which not only prevents the multiplication of the bacteria, but may even kill them.

On the whole, the burden of proof rests decidedly upon the advocates of the phagocyte theory. We are plainly not in a position at the present time to frame an adequate theory of immunity. There is no convincing evidence of any struggle of the leucocytes with the living bacteria. The existence of the cell militant cannot as yet be recognized. On the contrary, there is a large and increasing amount of testimony against the possession of the soldier function by the white blood-cell.

While the theory that the leucocyte is the defender of the organism, and the theory that it is the agent by which fat is taken inside the body, have been by far the most prominent of recent views concerning the function of the white blood-cell, there is still another way by which it has been thought that the leucocyte might make good its claim to be considered a helpful member of the organism. This is Cohnheim's theory of the formation of scar-tissue. Cohnheim, supported by Ziegler and other accurate observers, held that the leucocytes which wandered into an inflamed area were not only the source of the pus cells when pus appeared, but were also the formative cells for the new tissue, if any new tissue were formed. This view has always been more or less opposed by some investigators in this field, and has recently been discussed afresh by Sherrington and Ballance.[15] These observers have

made a number of careful experiments on the formation of scar-tissue, and conclude that the theory that scar-tissue is produced by the transformation of leucocytes should be abandoned. "Our observations yield no support to Cohnheim's view of the genesis of cicatricial tissue from leucocytes."

The present seems indeed to be a time when the leucocyte is being denied what few virtues it was once allowed to possess, for Looss [16] has controverted *in toto* Metchnikoff's fundamental observation, that the leucocytes are the active agents in destroying the useless tissues of animals, — as the tail of a tadpole at the metamorphosis period. "Not only the muscles and nerves, but also the other tissues of the batrachian tail, are destroyed independently, and are digested by the fluids of the body without the help of the leucocytes."

Thus it is evident that, although the last few years have materially advanced our knowledge of the white blood-cell, and given rise to many ingenious theories concerning its function, we cannot accept any one of these theories as the whole truth and nothing but the truth. It is yet impossible for the physiologist to state positively the function of the white blood-cell.

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2 **Zawarykin**. Ueber die Fettresorption im Dünndarm. (Pflüger's Archiv, Bd. XXXI., 1883.)

3 **Schæfer**. Ueber die Fettresorption im Dünndarm. (Pflüger's Archiv, Bd. XXXIII., 1884.)

4 **Heidenhain**. Beiträge zur Histologie und Physiologie der Dünndarmschleimhaut. (Pflüger's Archiv, Supplement, Bd. XLIII.)

5 **Metchnikoff**. Intracellulare Verdauung. (Arbeiten aus dem zoologischen Institut zu Wien, Bd. V., 1883.)

Untersuchungen über mesodermale Phagocyten einiger Wirbelthiere. (Biol. Centralbl., III., No. 20.)

Sur la Lutte des Cellules de l'Organisme contre l'Invasion des Microbes. (Annales de l'Institut Pasteur, 1887, p. 321.)

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Études sur l'Immunité. (Annales de l'Institut Pasteur, 1889, p. 289.)

6 **Wyssokowitsch**. Ueber die Schicksal der in's Blut injicirten Mikroorganismen im Körper der Warmblüter. (Zeit. für Hygiene, Bd. I. p. 3, 1886.)

7 **Hess**. Untersuchungen zur Phagocytenlehre. (Virch. Archiv, Bd. CIX. u. CX.)

8 **Nuttall**. Experimente über die bakterienfeindliche Einflüsse des thierischen Körpers. (Zeit. für Hygiene, Bd. IV. p. 353.)

9 **Baumgarten**. Lehrbuch der pathologischen Mykologie, 1888, p. 90 *et seq.*

10 **Bitter**. Kritische Bemerkungen zu Metschnikoff's Phagocytenlehre. (Zeit. für Hygiene, Bd. IV. p. 318.)

11 **Christmas-Dirkinck-Holmfeld**. Ueber Immunität und Phagocytose. (Fortschr. d. die Med., Bd. V. pp. 401, 583.)

12 **Buchner** *et al.* Untersuchungen über die bacterienfeindlichen Wirkungen des Blutes und Blutserums. (Archiv für Hygiene, Bd. X. Heft 1, p. 84; Heft 2, p. 121.)

13 **Nissen.** Zur Kenntniss der bakterientödtenden Eigenschaften des Blutes. (Zeit. für Hygiene, Bd. VI. p. 487.)

14 **Lubarsch.** Ueber die bakterienvernichtenden Eigenschaften des Blutes und ihre Beziehungen zur Immunität. (Centralblatt für Bakteriologie, Bd. VI. Nos. 18, 19, 20, pp. 481, 529.)

15 **Sherrington and Ballance.** On Formation of Scar-Tissue. (Journal of Physiol., Vol. X. No. 6, p. 550.)

16 **Looss.** Ueber die Beteiligung der Leukocyten an dem Zerfall der Gewebe im Froschlarvenschwange während der Reduktion desselben. (Leipzig, 1889.) (Abs. Biol. Centralblatt, Bd. IX. p. 595, 1889.)

ON THE DETERMINATION OF CARBONIC ACID IN WATER, AND THE BICARBONATES OF LIME AND MAGNESIA.

BY ALLEN HAZEN, PH. B.

I HAVE tried various methods for the determination of carbonic acid in water, depending upon the precipitation of calcium carbonate, and also by boiling off and weighing in potash bulbs. The results obtained were not satisfactory; not only did the different processes fail to give concordant results, but duplicate determinations by the same process seldom agreed. The reason for these unsatisfactory results, as I shall show, is loss of carbonic acid during the collection of the sample and the measuring out of portions.

To overcome this difficulty, I have proceeded as follows. A graduated flask, having a considerable space in the neck above the mark, is filled from a tube passing to the bottom of the flask, and at least several hundred cubic centimeters of water are allowed to pass through and overflow the flask. This is easily done when the water is taken from a tap. In other places a different arrangement must be used. The precautions required are precisely the same as for the collection of samples for dissolved oxygen; the water must not at any time come in contact with air, or any other gas. The water in the flask is rapidly brought to the mark by means of a siphon, and lime-water, or caustic soda free from carbonic acid is at once added in such quantity that when mixed it will color phenolphthalein. A large excess is to be avoided. After mixing, the contents of the flask may be poured out without danger of loss, and the carbonic acid determined by well known methods depending upon the precipitation of calcium carbonate.

It is much easier, however, to titrate with acid, using first phenolphthalein, and after the pink color is discharged using methylorange as an indicator. The difference between the quantities of acid required with the two indicators represents one half the total carbonic acid. Instead of proceeding in this way it is found to be more convenient to add standardized alkali in slight excess, and titrate back with acid, using phenolphthalein as an indicator. Another portion of water is titrated directly with acid using methylorange. In this case the sum

of the acid used with methylorange, and the acid equal to the alkali required with phenolphthalein is equal to one half the carbonic acid.

This process depends upon the neutrality of bicarbonates to phenolphthalein. Sutton[1] makes use of this titration for the determination of carbonate in presence of caustic alkali. To determine the accuracy of the process, titrations were made of a solution of ignited sodium carbonate, and of a mixture of this solution with water, and with other salts. The water used was made free from carbonic acid by prolonged aeration with air which had first passed through caustic potash. A few results are as follows: —

Na_2CO_3 Solution.	Other Salt.	Total Volume.	$\frac{N}{20}$ H_2SO_4.		CO_2 found.	CO_2 taken.	Remarks.
			Phenolphthalein.	Methylorange.			
		c. c.	c. c.	c. c.	mgs.	mgs.	
		100	0	0.05	0.11	..	Distilled water.
c. c.							
0.5	....	100	0.25	0.55	0.66	0.54	
5.0	....	20	3.00	4.90	4.18	5.4	Acid added rapidly with little stirring.
5.0	....	50	3.00	4.90	4.18	5.4	" " "
5.0	....	100	2.60	4.90	5.06	5.4	" " "
5.0	....	5	2.80	4.87	4.55	5.4	Acid added slowly with constant stirring.
5.0	....	10	2.65	4.88	4.69	5.4	" " "
5.0	....	20	2.50	4.90	5.28	5.4	" " "
5.0	....	50	2.45	4.90	5.39	5.4	" " "
5.0	....	100	2.42	4.90	5.45	5.4	" " "
5.0	1.0 gr. $CaCl_2$	100	2.30	4.90	5.72	5.4	" " "
5.0	0.1 gr. "	100	2.45	4.90	5.39	5.4	" " "
5.0	1.0 gr. $MgCl_2$	100	2.45	5.00	5.61	5.4	" " "
5.0	0.1 gr. "	100	2.45	4.95	5.50	5.4	" " "
5.0	1.0 gr. NH_4Cl	100	0	4.90	..	5.4	" " "
5.0	0.1 gr. "	100	1.60	4.95	7.37	5.4	" " "
5.0	0.01 gr. "	100	2.45	4.95	5.50	5.4	" " "

[1] Volumetric Analysis, 5th ed., p. 46.

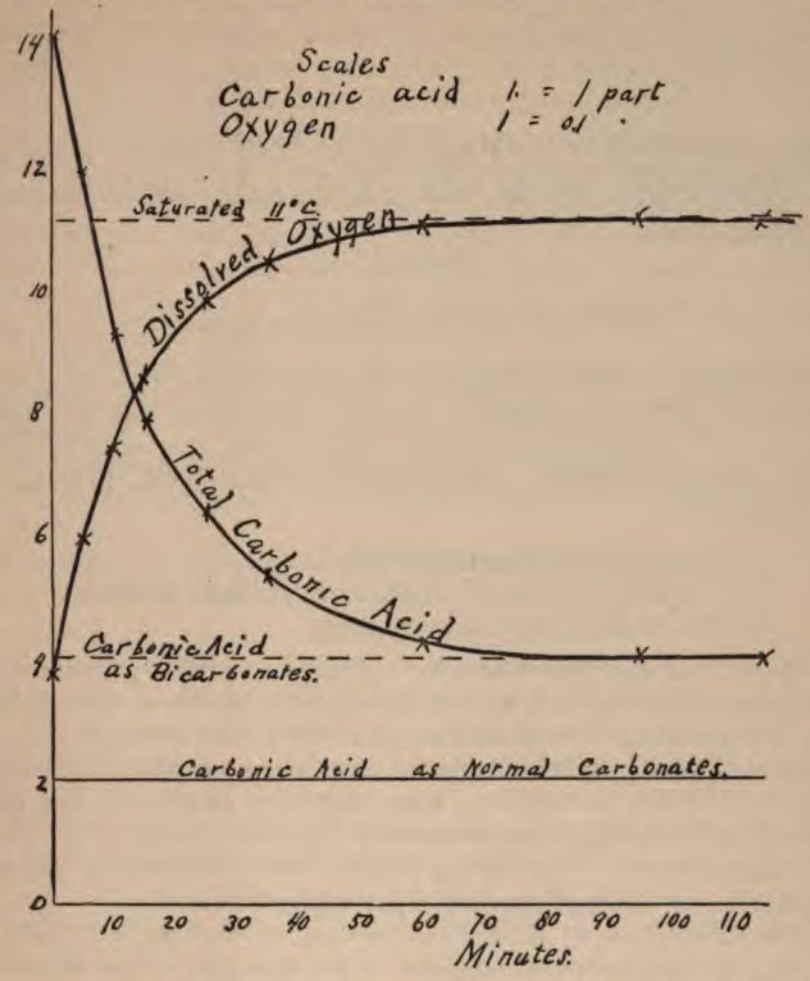
The low results with a concentrated solution of sodium carbonate are due to the loss of carbonic acid during titration from that portion of the surface of the liquid which is acid. This loss is greatly reduced by keeping the liquid thoroughly mixed, and by using a not too concentrated solution. The presence of calcium and magnesium chloride does not influence the result. Even with one per cent, fairly good results were obtained. Ammonia does not influence the result when present in as large quantity as is found in water or sewage. With a very large amount the result is too high.

This process thus gives good results, under suitable conditions, with sodium, calcium, magnesium, and ammonium carbonates. We may then fairly assume that the results obtained for waters are correct, especially when the alkali used is only in slight excess, as then there is almost no chance for loss of carbonic acid during the titration, which is by far the greatest source of error.

Water at 15° will dissolve about its own volume of carbonic acid, or 200 parts per 100,000 by weight. Atmospheric air contains only 4 parts of carbonic acid in 10,000, and therefore water saturated with air will contain only 0.08 of a part per 100,000, — a quantity too small to be determined. We should expect, then, that a water containing free carbonic acid would very rapidly lose it on exposure to the air, and this is the case. I have found that water which, taken from a tap in the manner stated, has 15 parts per 100,000 of free carbonic acid, will not show more than 8 or 10 parts when a bottleful is taken, and flasks are filled from it at once, by pouring. Naturally, no two results will agree; the more exposure, the lower will be the result. With the process described, it is possible to obtain an indefinite number of closely agreeing determinations.

By prolonged exposure, the carbonic acid is reduced to twice the equivalent of the free base present. This is well shown by the following experiment. A large bottle was filled with a natural water containing much carbonic acid and but little oxygen. A slow and regular stream of air was blown through, and, at frequent intervals, samples were siphoned out for the determination of carbonic acid and dissolved oxygen. The results obtained during the first two hours are shown by the following diagram.

Experiments have been made in the same way, using distilled water and solutions of sodium, calcium, magnesium, and ammonium carbonates in water with an excess of carbonic acid. The results are almost exactly like that shown by the diagram, and need not be given here. By continuing the air current for a very long time, a slight addi-



tional amount of carbonic acid is removed, and the water will then color phenolphthalein. The rate of removal of carbonic acid, after reaching the point where it is equivalent to twice the base, is many times slower than before. To this point the rate is equal to the rate of removal from distilled water containing as much carbonic acid as there is here excess over twice the equivalent of base.

These experiments show the existence of real bicarbonates of calcium and magnesium. They also indicate that these bicarbonates are slightly dissociated by water, as is also sodium bicarbonate in equal dilution.

LAWRENCE EXPERIMENT STATION,
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DATA AND PLOTS OF VARIOUS INCANDESCENT LAMPS, TOGETHER WITH AN IMPROVED METHOD OF TESTING.[1]

PART II.

BY WM. L. PUFFER, INSTRUCTOR IN PHYSICS.

THE object of this investigation was to determine the efficiencies of various makes of incandescent lamps. The method used was similar to, but in all respects an improvement upon, that used in 1885 by the Franklin Institute. The component measurements to be made are these: —

1. The current passing through the lamp.
2. The difference of potential between the lamp terminals.
3. The required photometer readings.

The current was determined by a Thomson current galvanometer placed in series with the lamp and an adjustable resistance for regulating the potential. The difference of potential was determined by a Thomson potential galvanometer of 7,500 ohms resistance connected with the terminals of the lamp. Great constancy of potential was rendered possible by the use of an automatic governor designed and built by the department. The current readings were corrected by deducting the current flowing through the potential galvanometer.

(The description of holder and method of working are omitted, as they are similar to those given in the first part of the paper. — W. L. P.)

The following varieties of lamps were tested in the order given: —

Lamps	C, D, M,	Sawyer-Man,	110	volts,	16	c. p.,	marked	16	c. p.,	A 3–16 c. p. A 4
"	A, B, L,	"	60	"	16	"	"	16	"	B 4
"	I, J, K,	Weston,	110	"	16	"	"	16	"	110 volts, B. B. G.
"	E, F, N,	Edison,	110	"	16	"	"	Edison's patents, 16 c., 3.1		
"	R, S, T,	"	93	"	16	"	"	"	"	16 c.
"	O, P, Q,	"	85	"	16	"	"	"	"	16 c.
"	G, H,	"	110	"	32	"	"	"	"	32 c., 3.1

[1] From Graduation Thesis on the Distribution of Light around Incandescent Lamps, with Efficiency Measurements, by Henry M. Hobart and Earl W. Gannett, Massachusetts Institute of Technology. These results and plots are taken directly from the thesis, with no

Three separate lamps of each kind were tested; two separate tests by different observers were made on each lamp, and their means taken to represent the values for that particular type of lamp.

Therefore, forty-two tests were made on twenty-one lamps of seven varieties. Every final result is the mean of the results for three separate lamps and for six separate tests. Every point plotted on the diagrams is the mean of twenty-four settings of the photometer disk. Four careful determinations of the value of "H" were made by the Kew magnetometer at the outset. The four results and their mean are .1747, .1751, .1744, .1748; mean, .1748.

The accuracy of the results was insured by checking, at three different stages of the work, the means of the results and the results of the means.

The following table and the first table on page 189 are taken from the thesis to show methods of combining results and the agreements.

PHOTOMETRIC OBSERVATIONS ON LAMP J, BEING A MEAN OF OBSERVATIONS J, BY E. W. G., AND J_2, BY H. M. H.

Angle.	Horizontal 0°.	Vertical 0°.	Vertical 45°.	Vertical 90°.	Vertical 135°.
0	14.90	15.13	24.28	25 68	20 46
30	20 13	15.13	22.63	24.25	18.45
60	25 70	11.93	15 41	17.73	13 33
90	27.73	8 58	8 85	9 18	9 00
120	23.23	11.25	15.60	18 18	13 81
150	17.58	13.60	21.53	25.15	18.88
180	14 91	15.38	24.45	27.10	20.40
210	20 03	15.13	21.83	22 73	18 13
240	25.60	11.46	14.78	14.13	10.53
270	27 83	0	0	0	0
300	22.86	9 03	12 08	12.46	12.50
330	17.53	14.28	21.03	22.71	18 03
0	15 13	15.18	24.23	25.88	20 55

correction or recalculations, which seemed unnecessary. I have added a few tables and words, which are in all cases accompanied by my initials. The work was intended for, and is, an extension of previous work, under far better conditions, and with the latest lamps obtainable. — WM. L. PUFFER. Reprinted from the Electrical Engineer, New York, April 2, 1890.

RESULTS OF OBSERVATIONS OF MESSRS. GANNETT AND HOBART, '89. — TABULATED AND ENLARGED. (W. L. P.)

Lamp. — 1888-89.	Mean standard reading.	Mean spherical c. p.	Spherical reduction factor.	Mean horizontal c. p.	Horizontal reduction factor.	Spherical divided by horizontal.	Volts.	Amperes.	Watts.	Watts per spherical c p.	Watts per horizontal c. p.	Lamps per electrical horse-power.	Mean spher. cand. per electric'l horse-p.	Mean hor. can. per electric'l horse-p.
Edison 85-volt, 16 c. p.	12.06	10.48	0.873	13.30	1 108	0.789	85.00	0.6200	52.70	5.04	3.98	14.14	148	188
Edison 93-volt, 16 c. p.	13.38	12.65	0.947	15.83	1.185	0.800	93.00	0 7240	67.33	5.32	4 26	11.07	140	175
Edison 110-volt, 16 c. p.	14.57	12.34	0.851	16.10	1.111	0.767	110.0	0.4552	50.07	4.07	3.12	14.90	184	240
Edison 110-volt, 32 c. p.	27.21	24.08	0.890	30.39	1.122	0.793	110.0	0 8507	93.58	3.89	3.08	7.97	192	242
Weston 110-volt, 16 c. p.	16.50	20.92	1.347	24.81	1.592	0.845	110.0	0.6451	70.97	3.47	2.93	10.51	220	261
Sawyer-Man 60-volt, 16 c. p.	23.54	19.06	0.810	23.39	0.994	0.815	60.00	0.9521	57.12	3.00	2.45	13.06	249	306
Sawyer-Man 110-volt, 16 c. p.	28.17	23.57	0.835	28.58	1.010	0.826	110.0	0.6724	73.97	3.17	2.62	10.09	239	288

EDISON 110 VOLT, 16 CANDLE-POWER, 3.1 WATTS. — MEAN RESULTS OF LAMPS E, F, N, AND THE MEAN OF THESE THREE MEANS.

	E.	F.	N.	Mean.
Mean standard reading	16.07	13.08	14.57	14.57
Mean spherical candle-power .	12.49	11.49	13.03	12.34
Spherical reduction factor . .	0.779	0.879	0.895	0.851
Mean horizontal candle-power .	16.16	15.33	16.80	16.10
Horizontal reduction factor . .	1.007	1.172	1.152	1.111
Ratio of spher. c. p. to hor. c. p.	0.774	0.751	0.776	0.767
Volts	110.0	110.0	110.0	110.0
Amperes	0.4705	0.4377	0.4574	0.4552
Watts	51.76	48.15	50.31	50.07
Watts per spher. candle-power .	4.156	4.191	3.862	4.070
Watts per hor. candle-power .	3.216	3.147	2.996	3.120
Resistance (hot)	233.8	251.3	240.5	241.9

1889 LAMPS IN ORDER OF EFFICIENCIES EXPRESSED IN TERMS OF WATTS PER SPHERICAL CANDLE-POWER. (W. L. P.)

	Maker.	Voltage.	Rated candle-power.	Watts per candle-power.	Candles per electrical horse-power.
1	Sawyer-Man	60	16	3.00	249
2	"	110	16	3.17	239
3	Weston	110	16	3.47	220
4	Edison	110	32	3.89	192
5	"	110	16	4.07	184
6	"	85	16	5.04	148
7	"	93	16	5.32	140

1889 LAMPS IN ORDER OF EFFICIENCIES EXPRESSED IN TERMS OF WATTS PER HORIZONTAL CANDLE-POWER. (W. L. P.)

	Maker.	Voltage.	Rated candle-power.	Watts per candle-power.	Candles per electrical horse-power.
1	Sawyer-Man	60	16	2.45	306
2	"	110	16	2.62	288
3	Weston	110	16	2.93	261
4	Edison	110	32	3.08	242
5	"	110	16	3.12	240
6	"	85	16	3.98	188
7	"	93	16	4.26	175

1889 LAMPS. SHOWING CANDLE-POWER ACTUALLY GIVEN OUT IN SPHERICAL CANDLE-POWER IN PERCENTAGE OF RATED CANDLE-POWER. (W. L. P.)

	Maker.	Voltage.	Rated candle-power.	Per cent.
1	Sawyer-Man	110	16	147
2	Weston	110	16	131
3	Sawyer-Man	60	16	119
4	Edison	93	16	79.1
5	"	110	16	77.1
6	"	110	32	75.2
7	"	85	16	65.5

1389 LAMPS. SHOWING CANDLE-POWER ACTUALLY GIVEN OUT ON THE HORIZONTAL CIRCLE IN PERCENTAGE OF THE RATED CANDLE-POWER. (W. L. P.)

	Maker.	Voltage.	Rated candle-power.	Per cent.
1	Sawyer-Man	110	16	178
2	Weston	110	16	155
3	Sawyer-Man	60	16	146
4	Edison	110	16	101
5	"	93	16	98.9
6	"	110	32	95.0
7	"	85	16	83.1

COMPARISON OF FOUR LAST PRECEDING TABLES, SHOWING POSITION OF LAMP WHEN COMPARED ACCORDING TO THESE METHODS. (W. L. P.)

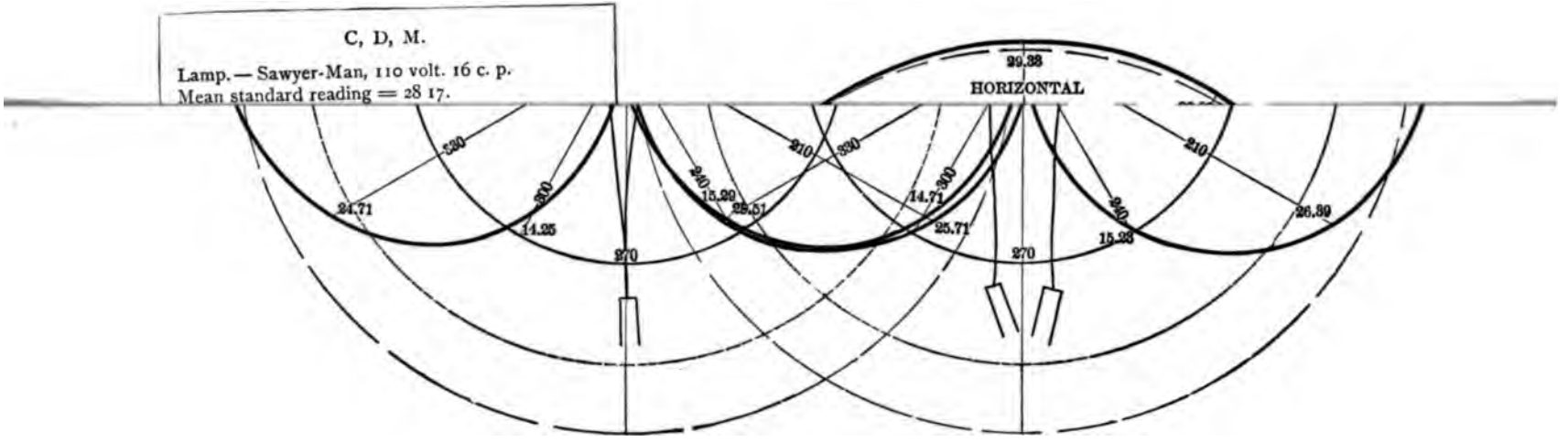
Maker.	Voltage.	Rated candle-power.	Watts per spherical c p.	Watts per horizontal c. p.	Per cent spherical c. p.	Per cent horizontal c. p.
Sawyer-Man	60	16	1	1	3	3
"	110	16	2	2	1	1
Weston	110	16	3	3	2	2
Edison	110	32	4	4	6	6
"	110	16	5	5	5	4
"	85	16	6	6	7	7
"	93	16	7	7	4	5

In addition to these results, it is necessary to know three more things before deciding which lamp is the best one, and the cheapest to use: 1. cost of new lamps; 2. life of lamp when burned at proper voltage; 3. fall of candle-power due to increase in the life of lamp.

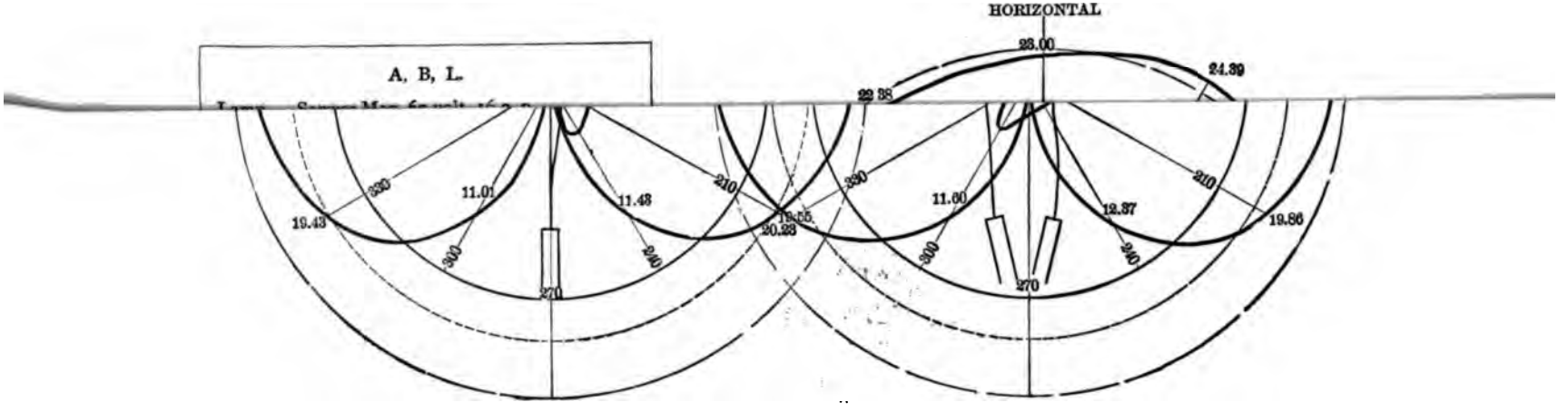
Under the cost of lamps must be reckoned the percentage of broken lamps which are always found in a batch of new lamps, the number which break instantly or within a few minutes of the time the current is first turned on, and those with defective fittings or otherwise in such condition that they cannot be burned. What experience I have had with lamps of the above kinds points to the conclusion, that from three to five per cent is about the proportion of loss; from actual count on one of the makes I find that I lose about five per cent of every new lot. This loss makes a very material difference in the cost of a lamp, and must be taken into account. As to the life of lamp and fall of candle-power with age, I have no figures ready for publication. It is a very open question whether a lamp's life ends when it ceases to give say 50 per cent of its original light, or when it becomes blackened, or when the filament actually breaks. From the moment the current is first turned on, the lamp begins to depreciate, sometimes increasing and sometimes diminishing in candle-power, or even both, until the filament finally breaks. I believe in a lamp that maintains its original light, or possibly increases it, until it has run a number of hours, and then fails rapidly and breaks, and not in a lamp which is at rated candle-power only for the first few hours of its life, and then dies by slow consumption at the end of its guaranteed life with a light only perhaps five or ten per cent of its original brightness, having lived as a "red-hot hair-pin" for the last half of its life.

It should be stated that all of the Weston and Sawyer-Man lamps were bought in Boston of the supply companies out of stock, and were in their original wrappers when taken for test. No questions were asked as to the use to which the lamps were to be put. Of the Edison lamps tested, the 110 volt 16 c. p. lamps came out of a stock of lamps on hand for general lighting use, but the rest were ordered by the local Edison company from the lamp factory, knowing that they were for testing purposes. Lamps could not be bought without a declaration of the purpose for which they were wanted.

Rogers Laboratory of Physics,
Mass. Inst. of Technology, *December*, 1889.


C, D, M.
Lamp. — Sawyer-Man, 110 volt. 16 c. p.
Mean standard reading = 28 17.
29.38
HORIZONTAL
210
330
210
330
240
15.29
25.51
14.71
300
14.71
25.71
240
15.23
26.39
24.71
14.25
270
270

THE ASTOR LIBRARY
NY


HORIZONTAL
23.00
24.39
22.38
A, B, L.
330
11.01
19.43
300
270
240
11.43
210
19.55
20.23
330
11.00
300
270
240
12.37
210
19.86

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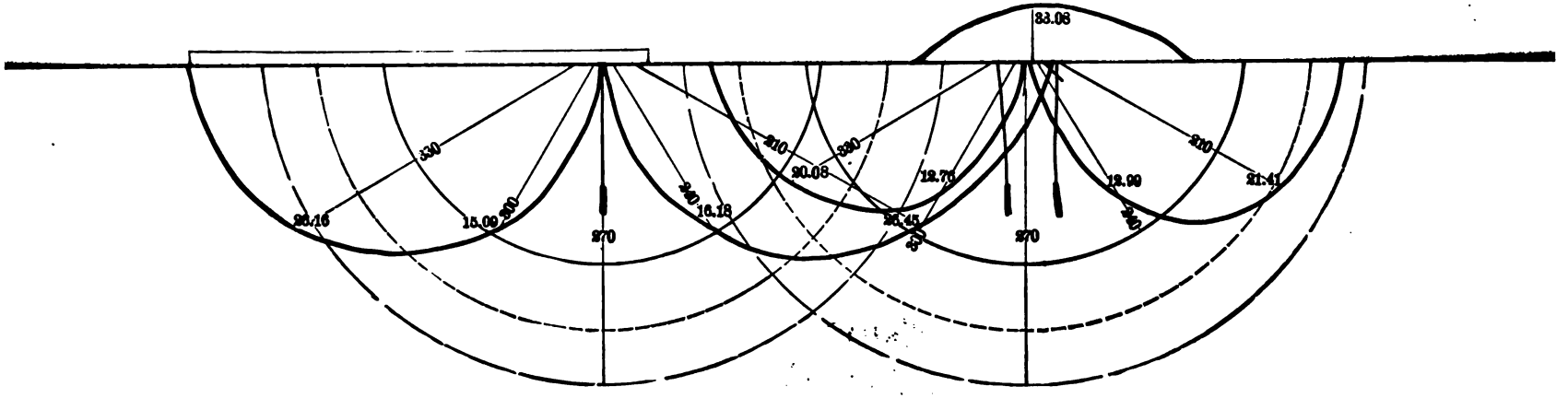
NY

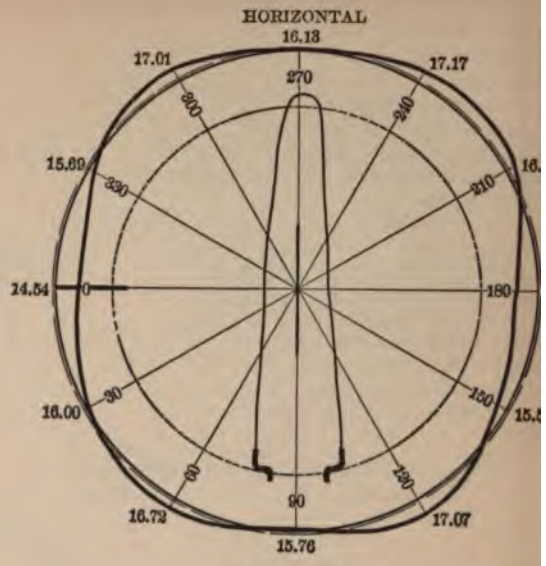

33.08
330
26.16
15.09
300
270
240
16.18
210
20.08
330
12.79
26.45
270
300
12.99
240
210
21.41

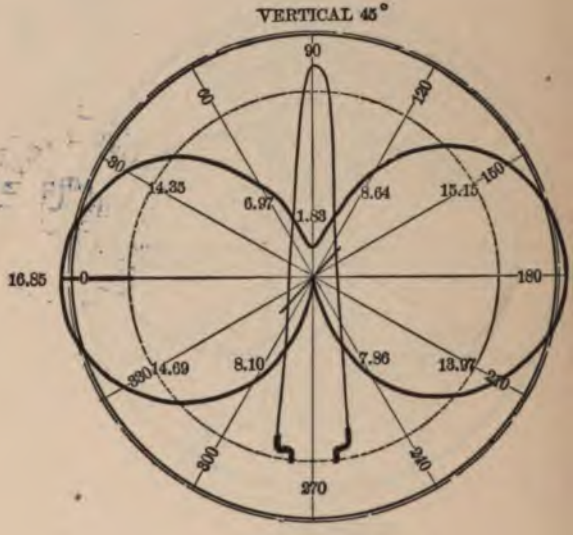
THE ASTOR LIBRARY
NY

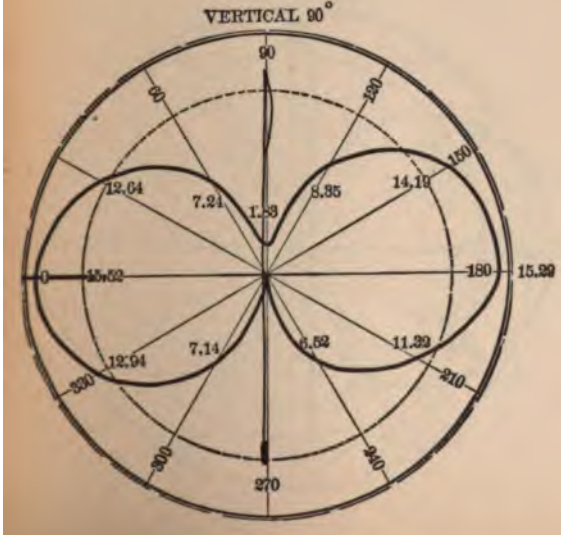
E, F, N.

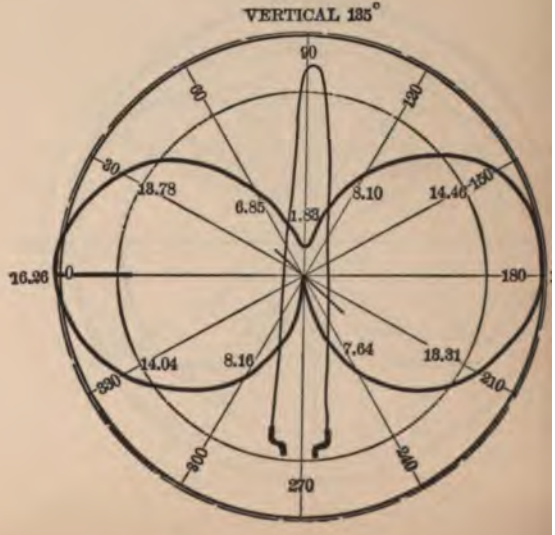
Lamp. — Edison new, 110 volt. 16 c. p. 3.1 Watts.
Mean standard reading = 14.57.
Mean spherical c. p. = 12.34.
Spherical reduction factor = 0.851.
Mean horizontal c. p. = 16.10.
Horizontal reduction factor = 1.111.
Mean sph. c. p. ÷ mean hor. c. p. = 0.767.
Volts = 110.0.
Amperes = 0.4552.
Watts = 50.07.
Watts per mean sph. c. p. = 4.070.
Watts per mean hor. c. p. = 3.120.
Rated c. p. = full circle.
Mean sph. c. p. = dotted circle.
Mean hor. c. p. = dashed circle.
Section of filament = Rect.
Scale of c. p. = 10 c. p. to inch.

Full circle reduced from 3.2″ to 2.5″ in engraving.









R, S, T.

Lamp. — Edison, 93 volt. 16 c. p.
Mean standard reading = 13.38.
Mean spherical c. p. = 12.65.
Spherical reduction factor = 0.947.
Mean horizontal c. p. = 15.83.
Horizontal reduction factor = 1.185.
Mean sph. c. p. ÷ mean hor. c. p. = 0.800.
Volts = 93.00.
Amperes = 0.7240.
Watts = 67.33.
Watts per mean sph. c. p. = 5.325.
Watts per mean hor. c. p. = 4.259.
Rated c. p. = full circle.
Mean sph. c. p. = dotted circle.
Mean hor. c. p. = dashed circle.
Section of filament = Rect.
Scale of c. p. = 10 c. p. to inch.

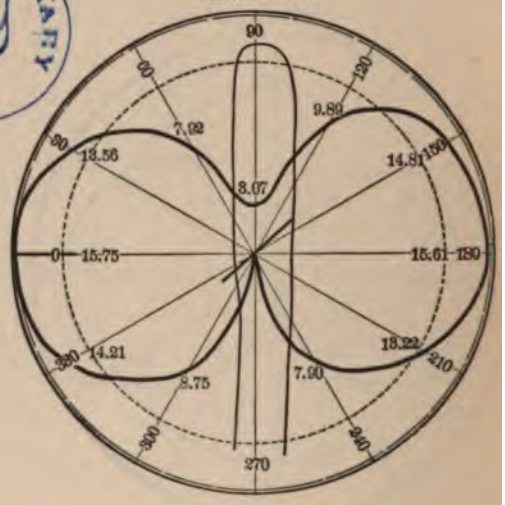
Full circle reduced from 3.2″ to 2.5″ in engraving.

HORIZONTAL

THE ASTON LIBRARY NY

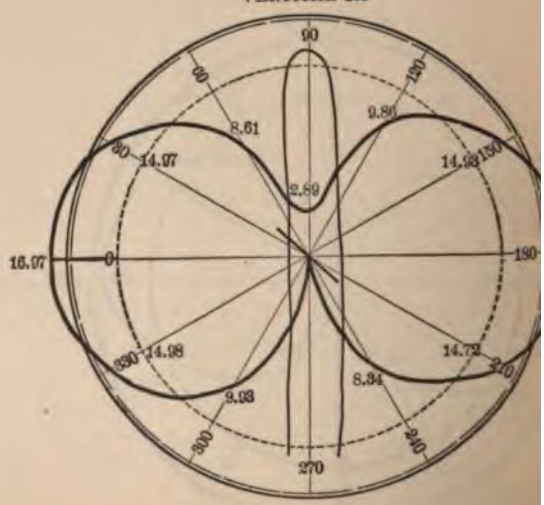
VERTICAL 0°

VERTICAL 45°



VERTICAL 90°

VERTICAL 135°

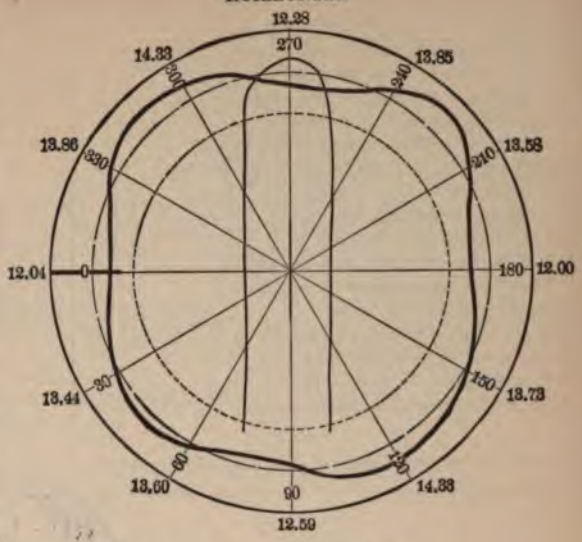


O, P, Q.

Lamp. — Edison, 85 volt. 16 c. p.
Mean standard reading = 12.06.
Mean spherical c. p. = 10.48.
Spherical reduction factor = 0.873.
Mean horizontal c. p. = 13.30.
Horizontal reduction factor = 1.108.
Mean sph. c. p. ÷ mean hor. c. p. = 0.789.
Volts = 85.00.
Amperes = 0.6200.
Watts = 52.70.
Watts per mean sph. c. p. = 5.044.
Watts per mean hor. c. p. = 3.976.
Rated c. p. = full circle.
Mean sph. c. p. = dotted circle.
Mean hor. c.p. = dashed circle.
Section of filament = Rect.
Scale of c. p. = 10 c. p. to inch.

Full circle reduced from 3.2″ to 2.5″ in engraving.

HORIZONTAL



VERTICAL 0°

VERTICAL 45°

VERTICAL 90°

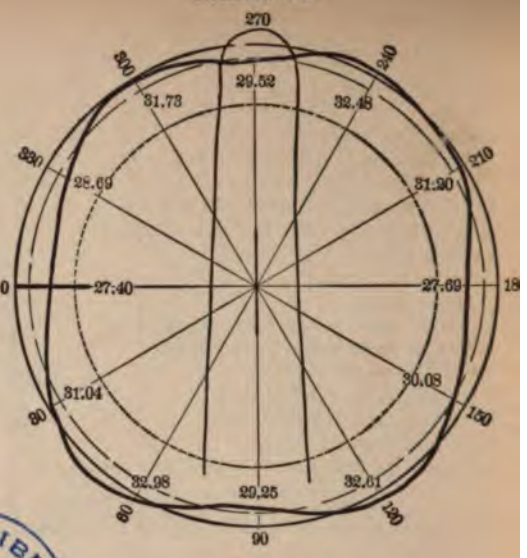
VERTICAL 135°

G. H.

Lamp. — Edison, 110 volt. 32 c. p. 3.1.
Mean standard reading = 27.21.
Mean spherical c. p. = 24.08.
Spherical reduction factor = 0.890.
Mean horizontal c. p. = 30.39.
Horizontal reduction factor = 1.122.
Mean sph. c. p. ÷ mean hor. c. p. = 0.793.
Volts = 110
Amperes = 0.8507.
Watts = 93.58.
Watts per mean sph. c. p. = 3.889.
Watts per mean hor. c. p. = 3.081.
Rated c. p. = full circle.
Mean sph. c. p. = dotted circle.
Mean hor. c. p. = dashed circle.
Section of filament = Rect.
Scale of c. p. = 20 c. p. to inch.

Full circle reduced from 3.2″ to 2.5″ in engraving.

HORIZONTAL



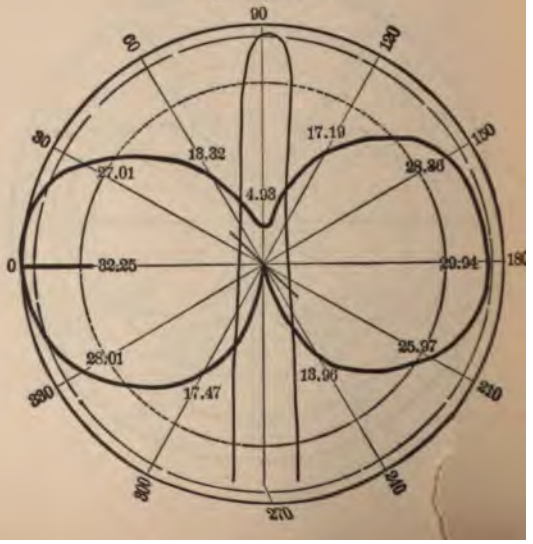
THE ASTOR LIBRARY NY

VERTICAL 0°

VERTICAL 45°

VERTICAL 90°

VERTICAL 135°



TECHNOLOGY QUARTERLY.

EDITED BY JAMES P. MUNROE.

VOL. III. AUGUST, 1890. NO. 3.

COMMEMORATIVE ADDRESS TO THE CLASS OF 1890.

(DELIVERED ON GRADUATION DAY.)

BY AUGUSTUS LOWELL, ESQ., MEMBER OF THE CORPORATION.

GENTLEMEN AND LADIES, — Twenty-five years have passed since this building was finished and the Institute of Technology was opened, upon a very modest scale and with a very small number of scholars; but the germ was there, and its rapid growth has come from allowing the school to develop by a process of evolution which has accommodated its teaching gradually and surely to the wants of the age, until, although far from perfect, it is to-day in completeness and comprehensiveness of technical instruction unsurpassed, if not unrivalled, by any institution of its kind.

The term "Technology" was given it by Dr. Jacob Bigelow, one of its earliest advocates, to represent a new departure in the history of instruction. Previous to this attempt, science had been taught as other branches of polite learning, by the university method, with little profit to the ordinary student; while those who made it their serious study prepared to devote their lives to its service, with no thought of applying it to the affairs of life. Science, pursued for its own sake, had little concern for the demands of trade. Any such partnership was beneath its dignity, and justly so. A great change was approaching, however, in the industries of the country. New fields of labor were to be opened up, based upon the discoveries of science and the inventions to which these had given rise, and a class of men would be needed to conduct these new industries for whose instruction no provision had been made in the established systems of education. To meet this want was the

aim of the founder of this school, Professor William B. Rogers. Himself a student of pure science and eminent in many of its branches, but gifted with a rare power rightly to interpret the coming movement, he threw himself enthusiastically into the work.

Professor Rogers was a native of Virginia. He had long been the Director of its Geological Survey, and Professor of Chemistry and Physics in its great University, but he had married here and had lately come to Boston to make it his home. To his high reputation as a scientist he added a rare power of lucid explanation, which enabled him to command the attention of all who listened to him, even when discussing the most abstruse subjects; and so simple and engaging was his manner that the fascination he exerted over all classes of men was irresistible. With these qualities he gathered about him a miscellaneous collection of persons distinguished in their several branches of business, but with little scientific knowledge or training. To them he explained the advantage which his science could bring to their pursuits, and invoked their public spirit to enable him to confer upon the community the inestimable blessing of such a school as he had conceived. Upon these men he relied for such advice and assistance in practical matters as he required, but the conception was his own, and for many years he not only planned, but carried on the work, supplementing the instruction, where needful, out of the fund of his own great knowledge and acquirements. It is not too much to say, not only that but for him would no such school have been founded, at least not at that time, but that it would have been impossible, with such a staff and such appliances as he had at command, to have held it together, had he not been endowed with so remarkable a talent for imparting the vast knowledge he possessed, and gifted with a power of sympathy and enthusiasm which affected all who approached him.

Having secured the interest of this knot of followers, Professor Rogers's next step was to obtain from the Legislature a charter and a grant of land. These were given in April, 1861, upon condition that fifty thousand dollars should be raised by private subscription to erect a building. The necessary sum was secured through the generous gifts of Dr. Walker and Mr. Huntington. The time, however, was not favorable to such an undertaking. The guns of Fort Sumter were still echoing in the public ear, and the demands upon the time and means of our citizens, and upon their interest and energies in the struggle for the maintenance of the Union, left little leisure or thought for other objects. The organization of the school was doubtless also hampered by the strangeness of the project, and the

incredulity of the public as to the demand for such instruction. It was not to be expected that it should at once meet with such public recognition as to secure for it a proper organization and equipment. The project was novel, and as it had been started by the foresight and public spirit of a few individuals, it must be carried on by them at their own expense and by their own exertions until the public mind should have become educated to appreciate the service it was prepared to render. It was clearly a case where the supply must be expected to create the demand. To-day, after the Institute of Technology has been sending its young men out into the world for a quarter of a century, trained to apply the principles of science to the exigencies of the useful arts, there is a tenfold greater demand for its graduates than before such a supply existed.

The close of the civil war found the people of this country ripe for rapid development. The excitement and bustle of war were to be replaced by such an exhibition of national energy in the peaceful arts as the world had never before witnessed. Great inventions, due to the scientific progress of the past fifty years, were crowding upon us. Hereafter entirely new classes of activity were to appear, and the youth of the country, in order to take their place in the coming change, must be specially equipped for the work.

In February, 1865, the school was opened with 27 scholars; the following year there were 72, and from this the number rose rapidly, until, in 1872, it had reached 348. Then came the financial crisis, with the loss of credit, depression of industry, and reduction of private incomes, and the effect of these upon the school was most disastrous. The number of students fell in three years to little more than one half what it had been in 1872, and this was followed by a forced reduction of expenditures, dismissal of valued instructors and a call upon those who remained to do so at a serious reduction in their salaries. The retrenchments rendered necessary at this time came near destroying the Institute of Technology. It had few graduates to speak for it, and they were young men, occupying subordinate positions. Its friends were almost entirely confined to Boston, and some of these began to distrust the enterprise which they had assisted in founding, and more than one meeting of the Corporation was held which came near resulting in the abandonment of the whole undertaking.

The excellent work of its earlier days of poverty and depression and the reputation of its graduates saved it. With the revival of business prospects in 1879 dawned a new era of prosperity for the school. Again the number of scholars began to increase. In 1880 it was

200, and from that time it has steadily grown, until now it has reached 900, and this with no lowering of the standard of scholarship either for admission or graduation. Nine hundred students, ninety permanent instructors, and eleven separate courses, each comprising many branches of instruction, mark the point we have reached to-day. Truly a wonderful result to have been accomplished in so short a time!

Such an unprecedented growth has called for new and ever increasing expenditures by the Corporation, for which the means have never been in hand. Thus the building put up in 1883 cost $300,000, for which there was only $63,000 specially provided. The debt at one time reached $200,000, and but for the courage and generosity of the treasurer, Mr. John Cummings, who put his own name upon its paper, it would have been impossible to go on.

To superintend the work of such an institution demands courage and foresight on the part of its Directors. To recognize the requirements of an enterprise of such rapid growth, and to provide in advance that there may be no hesitation or delay in carrying on the work efficiently, calls for business talent of no mean order; to do all this trusting to the future generosity of an appreciating public requires a reliance upon human generosity, and a blind confidence in the future, which find no place in the ordinary conduct of affairs.

But even with a wise and efficient government, failure must have ensued but for the generous co-operation of the Faculty. Patience, resolution, and self-sacrifice have all been required at the hands of the Professors and teachers, and nobly have they done their part. In the early days of the school the service of these men was a labor of love, but animated by their leader and encouraged by his devotion, they labored on, at every sacrifice, and earned a debt of gratitude from all lovers of higher education, which was the only return they could hope to receive beyond that of their own approval. In the name of the Corporation I desire to put upon record our full appreciation of all that they have done and are still doing for the Institute, and to thank them in our own name and that of the public for their faithfulness and generosity. I know they have a pride in their work. I know that they glory in the Institute; and well they may, for it is above and beyond all things the result of their conscientious and disinterested labors.

The debts of the Institute were paid in 1889, through the generosity of the Commonwealth, and by means of a large private contribution obtained chiefly through the exertions of Mr. William Endicott, Jr. But it was not destined to remain long out of debt. This condition

had hardly been reached before it became evident that the renewed demand for space, due not more to the increase of students than to the rapid specialization of the work of the Institute and the higher and larger types of machinery and apparatus, had rendered the extension of only six years before inadequate.[1] To meet this want the Corporation was compelled last summer to erect a new engineering building on Trinity Place, at a cost of $120,000, of which we owe practically the whole amount to-day. We have now ample space, but there is much that ought to be done, in justice to the teachers especially, which we would gladly do, but cannot for lack of means. The school has grown so fast, that it has been impossible to preserve its due proportions. Much has been pushed aside to meet more urgent wants, and we are sometimes almost tempted to wish that we could pause and put our house in order. But as success means growth, and rapid growth must necessarily be accompanied by much incompleteness of detail, we must accept its conditions, while we triumph in the result of our work, and remember that the school can never be complete until it has ceased to grow, and that then its days of usefulness will be numbered. Great, however, as have been and still are the needs of the Institute, it never has been our habit to proclaim our wants. We have rather chosen to rely upon the recognition of a service well performed, knowing that the public is never slow to appreciate what deserves support, and may be trusted to see that an institution from which it gains so much is not suffered to languish for lack of means.

An institution of learning may make a demand upon public recognition and gratitude because of its good work in training successive classes of young men for usefulness in life, even though it be not an innovator in education, and uses only the old and familiar methods of instruction; but it may acquire a further and larger claim by becoming a leader in its department, by introducing new methods, and opening the way to a better kind of intellectual and professional training.

How the Institute of Technology has dealt with the thousands of young men who have been its pupils since 1865, what it has done for them, what places they now occupy in the industrial system, what services they have rendered to the arts and industries of the country, common fame will tell. Those who would study this matter more

[1] The expense of instruction at a scientific and technical school increases far more rapidly, under an increase in the number of pupils, than at an ordinary classical college. The students must be taught in small sections, often in little groups around a machine or piece of apparatus, under the guidance of an instructor. The cost of delicate instruments and powerful machines, which must always be kept abreast of invention and the best professional practice, is also very great.

carefully will find material in the lists of its graduates and of the places they fill, as told in the annual catalogues.

But in addition to its work in training a certain number of young men for the duties of life, the Institute of Technology has been pre-eminently a leader in education. Its influence has not been confined to what it has done for its own pupils, but has extended as far as its example of advanced scientific and technical instruction has gone.

Almost at the very outset a long step forward was taken in the establishment of a laboratory of general chemistry. Up to that time general chemistry had been taught wholly by means of text-books, or by lectures with experiments by the lecturer. The student's part was only to look and to listen, and learn in this way what he could. It was not until the student was put into the analytical laboratory, and took the retort into his own hand, that he did or discovered anything for himself. Under the inspiration of Professor Rogers and the enterprise and administrative skill of Professor Charles W. Eliot and Professor Frank H. Storer, a laboratory of general chemistry was established, and the pupil from the first day of his chemical studies was set to teach himself. This was no analytical laboratory. It was simply designed as a means of illustrating, emphasizing, and supplementing the instruction of the lecture-room in regard to the nature of chemical action and the characteristics of the principal elements. The student was not told what he should find. He was told to do something and note what occurred. He was thrown upon his own faculties of observation and reflection. He learned to know himself, and to measure his own power, and he acquired ease and accuracy of manipulation by practice. So far as known, this was the first laboratory of such a character set up in the world. Certainly it was the first one instituted in the United States for the instruction of considerable classes of pupils. The publication of Eliot and Storer's Manual, designed for students taking this course, marked an epoch in the history of education.

Another equally important step in scientific education, and one of which the originality is beyond doubt, was taken at about this time in the establishment of a laboratory now known as the Rogers Laboratory of Physics. Under the inspiration of President Rogers the scheme of a laboratory where the student of physics should be set to make observations and conduct measurements for himself, in demonstration and illustration of the physical laws taught in the lecture-room, was carried out with remarkable ability on both the scientific and administrative sides by Professor Edward C. Pickering, now Director of the Harvard Observatory. So complete was Professor Pickering's study

of the needs and capabilities of such a laboratory, so masterly his treatment of it, that it has required only more room and additional apparatus to allow the system he then devised and formulated to be extended successively to classes of fifty, of one hundred, and even of one hundred and fifty students.

In the school year of 1871–72 another forward step in education was taken at the Institute of Technology. Down to that time the instruction in mining engineering and metallurgy had been, here as elsewhere, conducted by means of text-books, lectures, drawing models, and assays of small pinches of ore, supplemented, in the case of the more fortunately situated schools, by occasional visits to mines in actual operation. In the year named a scientific expedition to the Rocky Mountains was undertaken by a large party of students and instructors from the Institute. While in the Colorado mining regions, Professor Runkle conceived the idea of a laboratory which should add to the existing means of instruction in mining and metallurgy the practical treatment by the students of economic quantities of ores. This conception, so fully in the line of the general work of the Institute, was given effect by the purchase in California, before the return of the expedition, of a number of pieces of apparatus suitable for the beginnings of such a laboratory. The apparatus thus obtained was set up by Mr. Robert H. Richards, then instructor, and now for many years Professor of Mining Engineering.

From these small beginnings made under Professor Richards's care it has grown steadily to this day. It was the first proper metallurgical laboratory devoted to the purposes of instruction in the world. It is under its title, "the John Cummings Laboratory," by far the largest and the best in the world to-day. Its graduates are found in the most important mines and smelting and reduction works of the United States, showing the effect of their training at the Institute, in which theory and practice were so happily combined, and in which everything taught in the lecture-room is at once put to use in experiment and research.

In 1873 a further step in technical education led to the establishing of a laboratory of steam engineering. An engine of sixteen horse-power was set up, and the necessary apparatus for engine and boiler tests was provided. Out of this humble beginning has grown the largest and best equipped mechanical engineering laboratory to be found, in which not only is the work of instruction carried further than ever before, but original research, conducted jointly by the students and their instructors, is pushed to points often beyond the range of ordinary expert investigation within the profession. In the same year

the Lowell Free School of Industrial Design was established at the expense of the Lowell Institute, for the purpose of promoting the industries of the country, and especially the textile manufactures, by cultivating the American taste in respect to form and color.

In 1876 the system of shop work as a means both of general and professional training was introduced. Half an acre of shops, filled with the best tools, machines, and engines, with over two hundred students pursuing this branch of instruction, represent to-day the poor, mean shed, with its scanty appliances, which was all that the funds at the command of the Institute allowed to be erected in 1876.

In 1881 was established a laboratory of applied mechanics, devoted especially to the tests of building materials in wood, stone, and iron. The equipment of the laboratory has been increased from year to year, until it comprises a great variety of apparatus and machines, designed largely by the instructors in that department, for making almost every kind of test which the purposes of the engineer, the architect, the shipbuilder, or the mill-owner may require, — beam tests, column tests, belting tests, rope and wire tests, shafting tests, tests by tension, by transverse strain, by compression, by tensile strain, and continuous, intermittent, or instantaneous tests.

In 1884 the germ of a biological laboratory, which had existed in a corner of the shed used for the workshops of 1876, was developed with the aid of a large amount of physiological apparatus. The resources of the laboratory were turned, first, upon the preparation of its students for subsequent medical studies, and, secondly, upon bacteriological investigations, to which the marvellous discovery of Koch and Pasteur had pointed. It is not too much to say, that there is scarcely a place in this country where as much important bacteriological work has been done during the past three years as in this laboratory of the Institute.

In 1882 the increased demands upon the department of physics for the higher and more technical instruction of students, looking forward to electrical practice, led to the establishment of a distinct service devoted exclusively to that end, and, in connection with the new building of 1883, to the equipment of an electrical laboratory, with engine, dynamos, electric motors, and a great variety of electric testing apparatus. Notwithstanding this equipment, this course in electrical engineering, as it has been developed at the Institute, could not be sustained but for the machinery and ample appliances of the engineering laboratories. The training of the electrical engineer at the Institute of Technology differs from that usually followed, in that the electrical

engineer is here regarded as primarily a mechanical engineer, but a mechanical engineer who has specially studied the mechanical requirements of the electrical industries and enterprises, just as the chemical engineer under the course established two years ago is regarded in his relation to the chemical industries. And this introduces us to the last contribution made by the Institute of Technology to the philosophy of scientific and technical education, in the recognition of laboratory work in mechanics as an essential feature of a proper training in any branch of the great engineering profession. In the mechanical laboratories the students in each branch of engineering, civil, mechanical, mining, electrical, chemical, and sanitary, are called to perform the work of experiment, and to deal with the generation of power, and its application to the exigencies of their several contemplated professions.

We have thus roughly traced the history of the Institute of Technology. We have seen within how few years it has grown from a doubtful experiment into one of the most important schools of the country. We have seen how largely it has enjoyed the confidence and liberality of the public, and we feel that we may securely rely upon the same generous support hereafter. We have seen how its methods of instruction have been adapted to the changes and developments of practical science. We have seen that in this mobility, this power of adaptation, lay the grand idea of the whole scheme, and we are sure that, so long as it continues to be its guiding principle, the Institute of Technology will stand; a monument to the character, learning, and wisdom of its founder, worthy the community in which its establishment was possible and by which it has been maintained, an honor to the instructors who have devoted their energies to its service, and fortunate, as we trust it may long be, under the direction of so distinguished and able a President as General Francis A. Walker.

ADDRESS OF PRESIDENT WALKER, UPON PRESENTING DIPLOMAS OF GRADUATION.

My Friends, — It is now my pleasant privilege, on behalf of the Corporation and Faculty of the Massachusetts Institute of Technology, to present to you the diplomas of your honorable graduation and to greet you Bachelors of Science. It is rightly a subject of congratulation, on your part and on the part of all these friends, that you have passed so faithfully and patiently through one or another of the severe courses which lead up to the degree of the Institute. Doubtless at times it has gone much against the grain to do all that was required of you, and to do it all well. But you have striven, and you have conquered. Never hereafter can you be as those who have not been tried. I do not believe that any one of you to-day regrets the severe exertions and the great sacrifices that have been required. Those who do such things never regret the doing.

Behind you is a long course of laborious and honorable study and achievement; before you, a world which will never make upon you a severer demand than you have already successfully met. Are you not, then, glad that you have taken so brave a start in the cool of the day, and made so long a march in the morning of your lives, and now find yourselves so far on the way to personal and professional success, while sluggards are still droning and dozing away in camp the hours when the sun is low and the air is fresh and sweet? You are not to-day indulging in vain regrets as to hours which have been misspent, opportunities which have been neglected. and time that has been wasted in folly or vice. With the rightful pride of those who have fought a good fight, have kept the faith and finished their course with honor, you stand here and now, on the threshold of the great world, in the consciousness of duty hitherto well performed, of preparation for the labors of life carefully and thoroughly made, and of suitable and ample equipment for all the responsibilities of professional practice in the several lines for which you have been qualifying yourselves. I congratulate you. We say farewell without sorrow, since it was for this you came to us. But we bid you farewell in all affection and respect, with all honor and regard, with the most pleasant recollections of your straightforward and honorable conduct here, and with the strongest anticipations of your success and prosperity, both in professional and in private life.

THE NEW HYDRAULIC LABORATORY OF THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

BY GEORGE F. SWAIN, HAYWARD PROFESSOR OF CIVIL ENGINEERING.

THE erection of the new engineering building of the Institute of Technology, to be occupied by the departments of Civil and Mechanical Engineering, offered an opportunity for a considerable extension in the Engineering Laboratories, and an attempt has been made to improve this opportunity by laying the foundation for a laboratory for hydraulic experiments, which should be so arranged as to permit of the carrying out of any experiments in hydraulics which it is practicable to perform within walls. Hydraulic experiments on a large scale must necessarily be performed out of doors, since the measurement of large quantities of water requires apparatus and appliances which cannot be accommodated within walls. Thus, the weir experiments of Mr. Francis, at Lowell, were made by taking the water from one of the canals, and using a lock as a measuring basin; those of Messrs. Fteley and Stearns, at South Framingham, were made by using a portion of the Sudbury River Aqueduct as a measuring basin; the orifice experiments of General Ellis, at Holyoke, were made in connection with the fall between two levels of the canal at that place; and the recent elaborate and careful experiments by Mr. Freeman on the flow of water through fire hose, the discharge of nozzles, and the height of jets, were made at Lawrence, where the hydrant system of one of the mills, as well as the city water supply, could be made use of.

But while experiments such as these are clearly excluded from among those which can be made in connection with a hydraulic laboratory in an institution of learning, there remain a large number which can properly be conducted within doors with the aid of suitable apparatus, and which, though they may be on a small scale as regards the quantities of water employed, nevertheless offer a large field for scientific investigation. The new laboratory of the Institute, as already stated, has been planned with a view to affording opportunity, as the work is extended, for carrying an any experiments which are thus practicable; that is to say, in the following directions: —

1. Experiments on the flow through orifices of small size, both free and submerged, and either sharp-edged, rounded, or fitted with

inside or outside mouth-pieces of various kinds, and under heads ranging as high as above seventy feet.

2. Experiments on the flow of water over weirs of small size, either free or submerged.

3. Experiments on the loss of head in small pipes of various kinds.

4. Experiments on the loss of head due to bends, curves, valves, diaphragms, or other obstructions causing sudden changes of velocity.

5. Experiments on the distribution of velocity in different parts of a liquid cross-section, either of a jet from an orifice, of a sheet discharged over a weir, or of a liquid flowing in a pipe.

6. Experiments on different water meters, including Mr. Herschel's Venturi meter, as well as the ordinary forms in the market.

7. The testing of small turbines and of various other small motors.

8. Experiments on the pressure of jets against plane or curved surfaces, and on the resistance of standing water to the motion of surfaces of different shapes through it.

9. Experiments on the siphonage of traps, and on other matters connected with plumbing arrangements of houses.

The development of a laboratory which shall admit of experiments in all these lines must necessarily be slow and expensive, and the laboratory in the Institute, being only a few months old, is not yet fully equipped to carry on any of these experiments excepting those on the flow of water through orifices. It is believed, however, that the foundation has been laid for the rapid development of research in the remaining directions which have been enumerated, and the object of the present paper is briefly to describe and illustrate the apparatus thus far provided or proposed.

In order to be able to work with large heads, and to be independent of the city water supply, as well as to provide for the varied experiments enumerated, it was first necessary to erect a tank and standpipe. The tank is shown in Figure 1. It is made of "Shell" steel $\frac{1}{4}$ inch in thickness, is 5 feet in diameter and about 28 feet high, resting on a concrete foundation and extending up through two floors. It consists of six courses of steel, with girth seams single riveted, and longitudinal seams double riveted, and with heads of $\frac{7}{16}$-inch steel dished as shown in the figure. It is provided with orifices as follows: —

In the lowest course, a man-hole 24 inches by 12 inches at M; at P, a flanged nozzle with elbow for connecting to 10-inch standpipe, as shown in Figure 15.

In the second course, a 10-inch orifice at G, a second at F, a third at D, and a fourth at C. The orifice at F may be used, if desired, for

connecting with a small turbine placed below the floor. That at G is for experiments on submerged orifices. Those at D and C are for experiments on free orifices, or for connecting lines of pipe with the tank. Enclosing the orifice at G, an angle-iron and two bent plates are attached to the side of the tank, as shown, to which a wooden tank extending horizontally and resting on the floor is to be attached. In this wooden tank will be placed a weir, and the water will flow through the submerged orifice at G, or through a submerged mouth-piece, either inside or outside, and either converging or diverging, and will then flow over the measuring weir. The orifice C is fitted for experiments on free orifices, as will subsequently be described. The orifice D is to be fitted with a piece to which pipes can be attached as desired, thus enabling the losses of head at diaphragms, valves, curves, etc. to be studied. On the same level with the orifices D and G connections are made for mercury gauges.

In the third course, a 1¾-inch orifice at G″ and another at C″, nearly above the large orifices G and C. These small orifices are for the shafts of the hand-wheels for raising the gates over the large orifices, as will presently be explained.

In the fifth course, orifices similar to those in the second course; and in the sixth course, orifices similar to those in the third course; thus rendering it possible to carry on experiments simultaneously upon two floors of the building. The top of the tank is provided with a flanged nozzle for connecting with the 10-inch standpipe, as shown in Figure 15.

The size of the tank is such that, with the orifices which it will be practicable to use, the velocity in the tank will be so small that it may be neglected, and the disturbance due to the inflowing water from the standpipe will also be small. Nevertheless, two gratings have been arranged, one at the top and one at the bottom. These gratings consist of plates of ⅛-inch iron perforated by ½-inch holes about an inch apart. They are made in three pieces, and rest upon angle-iron brackets riveted to the inside of the tank. The tank itself rests upon cast iron supports, as shown in Figure 1.

The general arrangement of the tank and standpipes is shown in Figures 15 to 18. Figure 15 shows the plan of the sub-basement, with the location of the tank, the rotary pump, and the steam pump. The tank *a* is connected, as shown, by the top connection *e* and the lower connection *f* to the 10-inch wrought iron standpipe *b*, which, as shown in Figure 17, extends to the top of the building, and is closed at the top. The connection *f* is tapped by the small pipe *g*, by means of

which the water may be drawn out of the system into the cistern *h*. The pumps take water from this cistern *h* and deliver it into pipe *k*, which, as shown in Figure 17, is arranged to carry it directly to the standpipe through the valve *s*, or to the 3-inch pipe *c* through the valve *t*. For use in the hydraulic experiments, the valve *s* is closed and the valve *t* opened, the water being thus delivered into the 3-inch pipe *c c*. From this pipe the flow into the 10-inch pipe is regulated by the valves *n*, which enable the head in the 10-inch pipe to be maintained at an almost perfectly constant level. Any excess of water overflows through the pipes *m* into the discharge pipe *d*. The pipe *c* therefore serves as a regulator of the head, and it has been found to fulfil its purpose admirably. Attached to the standpipe *b*, and running from floor to ceiling, in each story, is a glass gauge placed in front of a graduated scale reading to hundredths of a foot. These gauges are enclosed in wooden boxes, which may be opened on the front and on one side, and which are placed so as to receive light directly from adjacent windows. The water is taken by the pumps from the cistern *h*, passes through pipes *k* and *c* and through the valves *n* into the standpipe *b*, thence through the connection *e* or *f*, or both, into the tank, from which it is discharged through orifices, or in any other way desired, and the discharge measured. Measurements are made by weighing the quantity of water discharged in a given time, the water being returned to the cistern *h*, and thus used over and over again.

The same apparatus, by suitable arrangement of the valves, will be employed in making tests of the pumps.

The city water pipe is connected with the standpipe system at *x*.

The soil pipe *dd* is arranged with single and double Y's in a way which will allow the carrying out of all kinds of experiments on the siphonage of traps, as mentioned under head 9 on page 204. This pipe is arranged with an ordinary trap at the foot, as shown in Figure 16, and it discharges into the cistern *h*, as shown in Figure 15.

The details of the arrangement for measurements with simple free orifices are shown in Figures 2 to 14. In experiments of this description it is of course essential that the orifice shall be placed in a plane surface. It was therefore necessary to arrange the apparatus so that the curvature of the tank itself should not affect the flow. For this purpose, a composition casting *a* (Figs. 2 and 3) is drawn up to the tank by eight $\frac{5}{8}$-inch bolts, as shown in Figure 3, which are screwed into the hub of the casting. The greater part of the casting is only $\frac{1}{4}$ inch in thickness, with eight strengthening ribs, as shown in Figure 2. In this casting is placed a piece, c_1, which is held in position by a ring-

nut, *dd*, which can be screwed or unscrewed by a spanner. When it is desired to use large or long orifices the piece *c* will contain the orifice, and by having various pieces *c*, with orifices of different shapes and sizes, numerous experiments may be carried out. The composition casting *a* is thickened on one side to ⅜ inch, as shown in Figures 2 and 3, to allow of the insertion of a sliding piece by means of which the horizontal dimension of a rectangular orifice may be varied, keeping the vertical dimension constant. When small orifices are to be used, it is not desirable to have them cut in as large a piece as the piece *c*. This piece *c*, therefore, as shown in Figure 3, is arranged to take a second piece, *o*, held in place by a second ring-nut, *d'*. Small orifices are made in pieces like the one *o*, as shown in Figure 3, and are of different sizes and shapes.

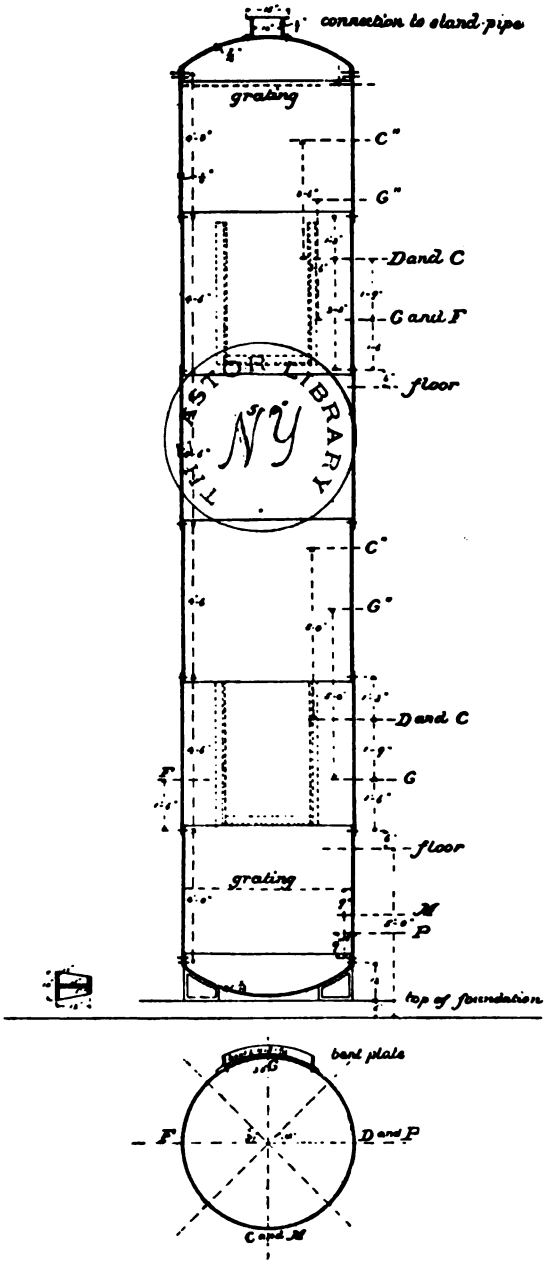
It is desirable that one orifice may be removed and another substituted in its place without completely emptying the tank. For this purpose, a gate is designed to slide on the back of the casting *a*, so that, when it is desired to remove one orifice, the gate may be lowered and the water thus shut off, and a new orifice substituted in place of the old one. The gate may then be raised and the experiments continued. This gate and the fittings connected with it are shown in Figures 4 to 14. Figure 4 shows a view of the gate from the inside, Figures 5 and 6 horizontal cross-sections, and Figure 7 a vertical cross-section. The gate is of cast iron, ribbed as shown, and is arranged to slide in guides bolted at the bottom to the casting *a*, and at the top to the tank. These guides are shown in Figures 8 to 11. In order that the gate may be raised without having to overcome the friction due to the pressure of the water over the entire surface of the gate, the rod *r*, by which the gate is raised, is attached to an angle-iron, *L* (Figs. 4 and 7). When the gate is to be raised from the position shown in Figure 7, the angle-iron *L* is first raised until it strikes against the rib above it. This raising of *L* opens the orifice *d*, thus allowing water to pass through the gate, and equalizing the pressure on the two sides, except over the small ring forming the bearing surface, the friction due to the pressure on which is, therefore, the only friction to be overcome. The discharge orifice may be meanwhile closed by a plug from the outside, if necessary. When the gate is lowered, its own weight carries it down until it reaches the stop *s*. The rod is then forced down, closing the orifice *d*. Figures 12 to 14 show the hand-wheel, shaft, and stuffing box, by means of which the gate is raised, and require little explanation. The rod shown in Figure 7 is in its upper part a rack running in the space *r* (Fig. 12).

Only one further point calls for explanation regarding the arrangement of the gate and stuffing box, namely, the modifications necessary for the case of inside mouth-pieces. When such mouth-pieces are used, the gate as shown would of course be inapplicable, and a new seat must be provided for it, so that it will pass clear of the inside mouth-piece. It is intended to accomplish this by bolting on to the composition casting *a* (Fig. 2) a circular channel-shaped rib, and adding new guides for the gate, thus making its seat at any desired distance back of the inside face of the casting. At the same time, in Figure 12 the pinion will be removed from the position shown, and placed outside of the bearing, where the collar is shown in the figure, and the collar placed on the inside. A new guide for the rack will then be bolted on, as shown by the dotted lines.

As the new engineering building has been occupied only since February, 1890, and the hydraulic apparatus is thus but a few months old, the only apparatus thus far in use is that for measuring the flow through simple, free orifices, and for making tests of pumps. It is hoped, however, that the remaining apparatus will be provided in the near future. The tank and the standpipe system already procured furnish the foundation, and upon them the other apparatus may readily be built up. Experiments upon meters and upon motors may also be carried out without difficulty; and, in fact, the possible lines of research are so varied that it is hoped that the establishment of this laboratory may lead to many useful results, and that it may contribute in some measure to the advancement of hydraulic science.

Steel Tank

Fig. 1.



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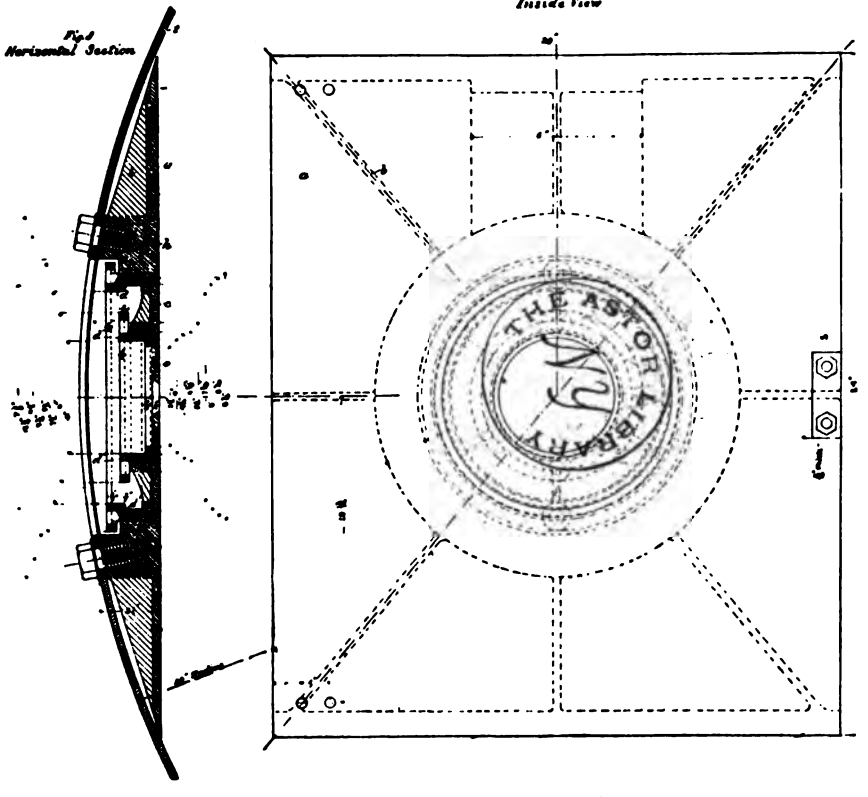
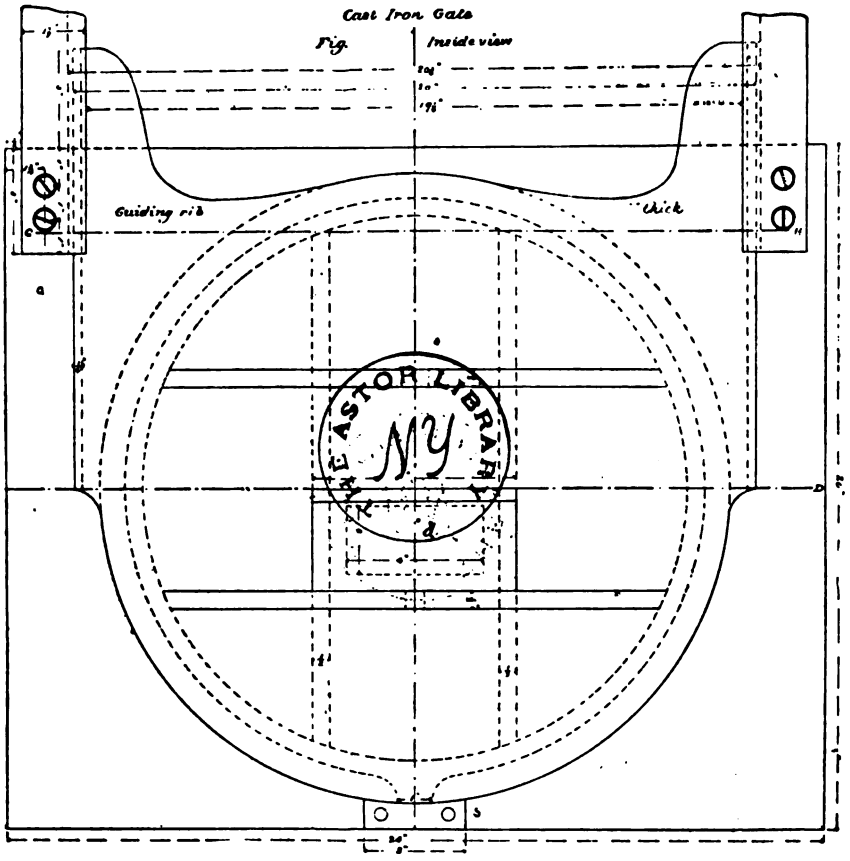
Details of Drum Casting





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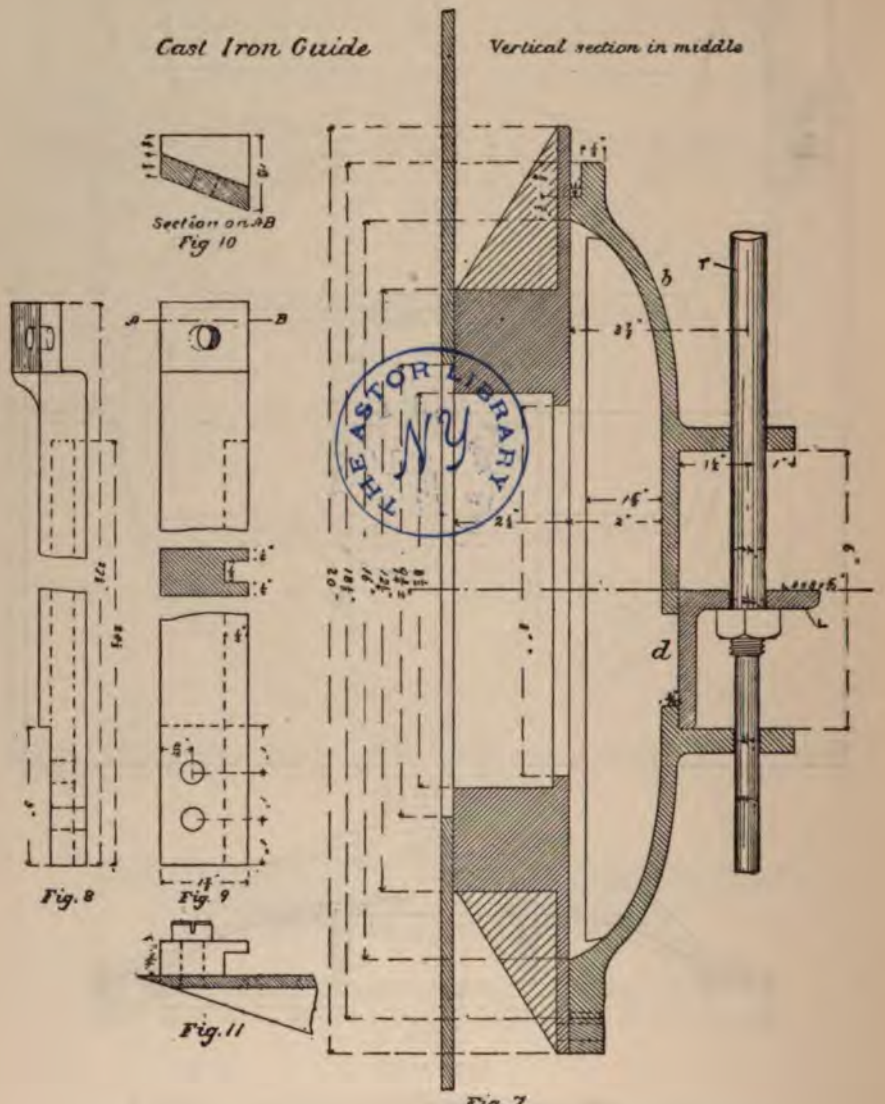
Cast Iron Guide
Vertical section in middle
Section on AB
Fig 10
Fig. 8
Fig. 9
Fig. 11
Fig. 7
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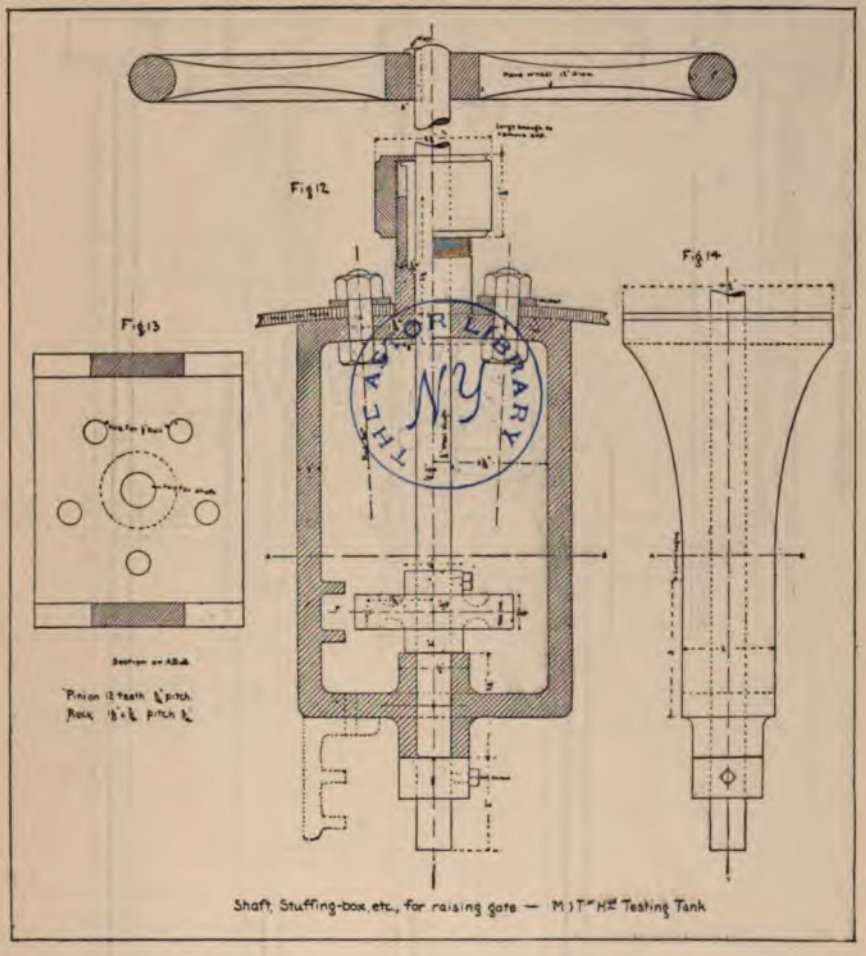
NY



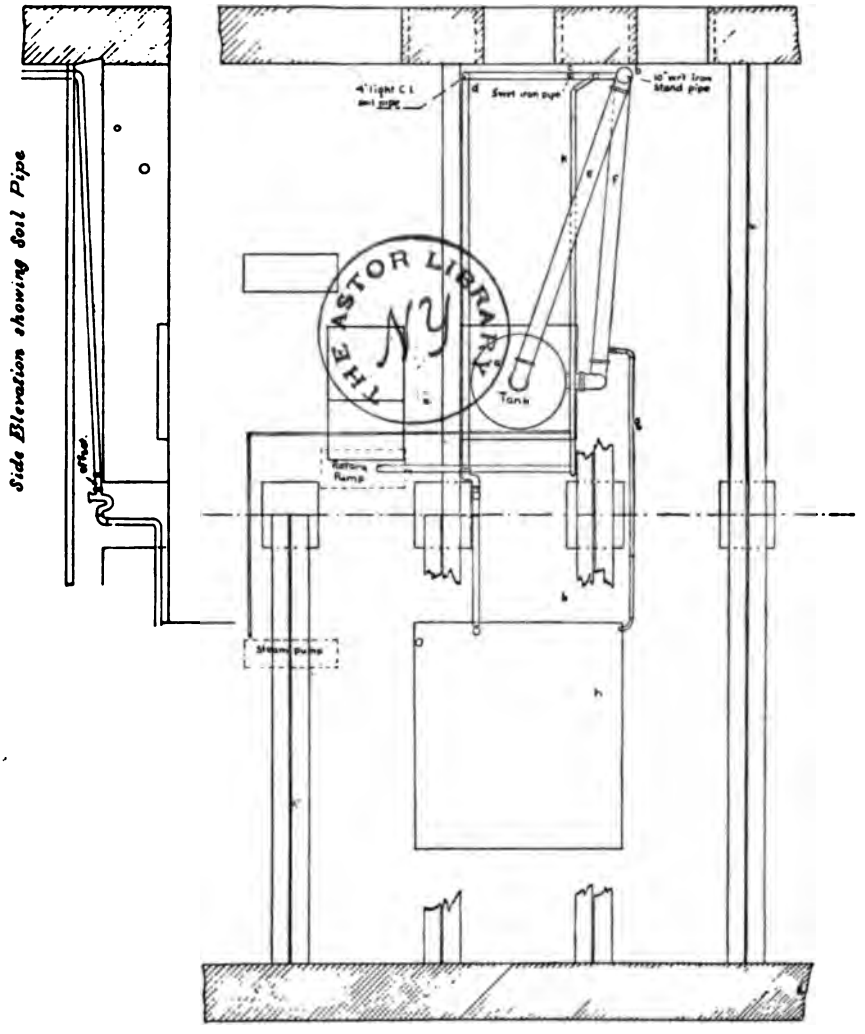
Shaft, Stuffing-box, etc., for raising gate — M.I.T. H.E. Testing Tank

Fig 15

Fig 16

Plan of Sub-basement

Side Elevation showing Soil Pipe



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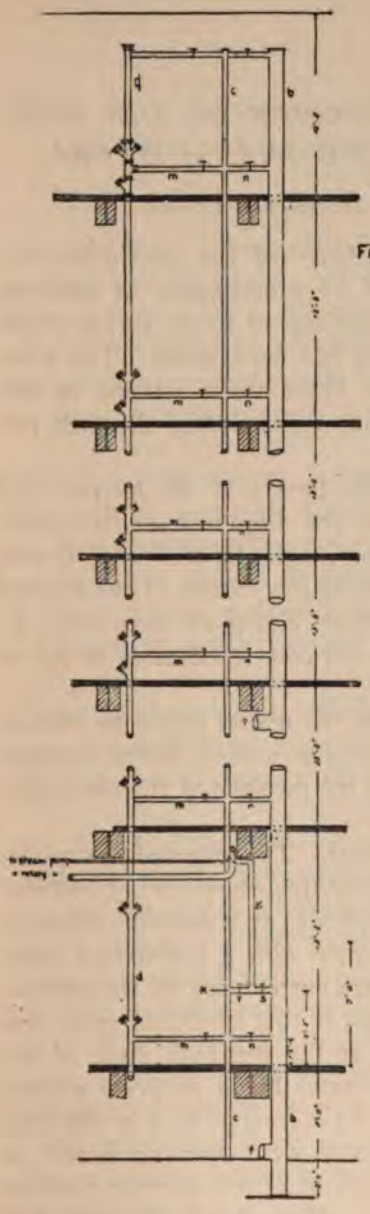

Fig 17

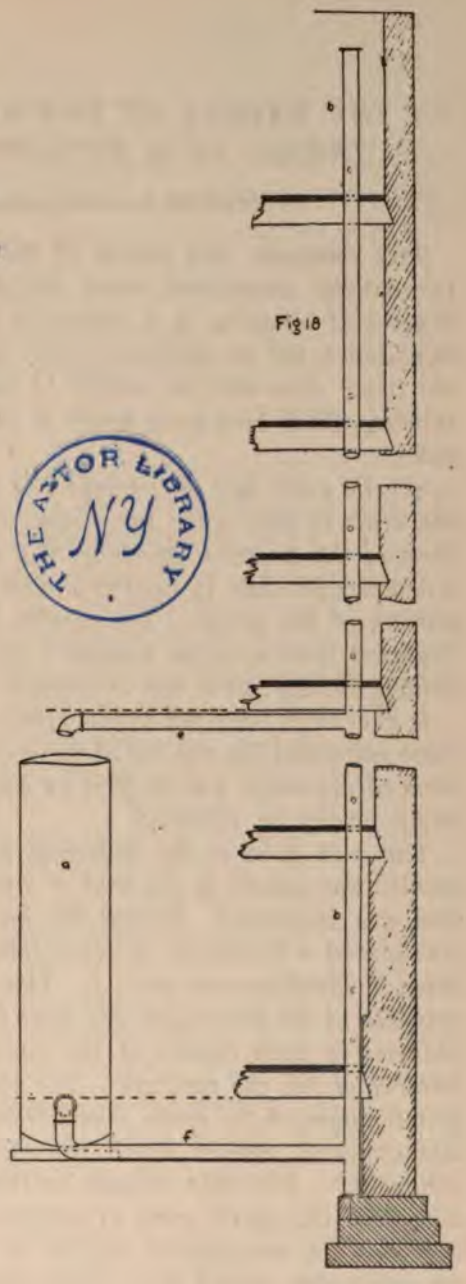

Fig 18

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ON THE EXTENT OF THE EXCURSION OF THE ELECTRODES OF A MICROPHONE TRANSMITTER.[1]

BY CHARLES R. CROSS, THAYER PROFESSOR OF PHYSICS.

THE character and extent of the motions of the electrodes of a microphone transmitter, when actuated by sound-waves of different degrees of intensity, is a subject in telephony of by no means slight importance, but to which very little study has been given. The present paper describes the results of some observations relating to this subject, which have been made at various times during the past two years.

Several years ago an attempt was made by Mr. W. W. Jacques and the writer to gain some knowledge as to the amplitude of the vibrations of the hammer electrode of a microphone, by observing it with a microscope while in operation, and noting the extent of the blurred portion of the image. The results, though giving all that could be expected from so crude a method, were not very satisfactory so far as definite measurement was concerned.

It afterwards occurred to the writer that the matter might be studied more completely by the use of the stroboscopic method, and an arrangement of apparatus was adopted by which the motions of the electrodes could readily be observed.

This was done in the following manner. The microphone to be studied was placed in the field of a microscope, whose line of collimation was horizontal. Behind the microphone, at a suitable distance, was placed a Helmholtz mercury interrupter, with a tuning-fork making 128 vibrations per second. The extra current due to the electromagnets of the interrupter was quite large, so that a brilliant spark was obtained at each rupture of the circuit, as the platinum style of the interrupter left the mercury. The interrupter being properly placed, and the light of the spark concentrated by a lens, quite a bright field was obtained, against which the electrodes were seen projected, as silhouettes. The light, though intermittent, of course seemed continuous, since the sparks were so numerous. When good definition was obtained, the microphone was set in operation, usually by means of an organ-pipe placed at a convenient and variable distance, and in

[1] Read at a meeting of the American Academy of Arts and Sciences, April 9, 1890.

some experiments by the voice. The pipe was blown by a constant blast, and great uniformity of intensity in the sound was secured. An open C_3 organ-pipe making approximately 256 double vibrations per second was commonly used, its pitch being variable to a moderate extent by shading the mouth or the opening at the top.

So long as the pipe was an exact octave in pitch above the interrupting fork, the electrodes of the microphone as seen with the microscope appeared to be at rest; but if the interval was slightly disturbed, the stroboscopic effect was observed, and the electrodes seemed to move slowly through their complete course. The rate of this apparent vibration was of course dependent upon the deviation of the pipe from exactness in its interval with the fork, and could be varied at will within quite wide limits.

The extent of the motion of the electrodes could be determined by observing the grains of dust which adhered to them, or some definitely marked roughness on their surface. Measurements were made by means of a spider-line micrometer, the wires of which were placed at a convenient distance apart, and the amplitude of the motions of the selected points of reference on the microphone was determined by estimating the relation of their apparent motion to the distance separating the wires of the micrometer. This last distance was frequently varied to diminish the liability to error from a possible bias of the observer towards an agreement with earlier results.

In the experiments described in the present paper, the electrodes were generally so adjusted that the motion of the anvil electrode was too small to be observed. Under these circumstances the observed motion of the hammer electrode, as measured by the micrometer, was the motion of this relatively to the anvil electrode, which is of course the quantity to be determined, rather than the actual excursion of the hammer electrode.

The microphone was placed in circuit with a battery and the primary of an induction coil, whose secondary contained a receiving telephone. With this arrangement the effect on the ear of the electrical variations due to the various values of the excursions of the electrodes could readily be observed. In many cases a second observer was stationed at the receiving telephone, which was then placed in a separate room.

Magnifying powers were used of from 50 to 1,000 diameters. With the higher powers the use of an objective of short focal length was difficult on account of the small working distance, so that, although even as short a focus as $\frac{1}{10}$ in. was sometimes employed, it was found

much preferable to obtain the needed magnification by the use of short focus eye-pieces.

The brilliancy of the electric spark was amply sufficient for illumination of the field, even with the highest magnification employed; a fact which calls attention vividly to the enormous "instantaneous intensity" (so to call it) of that light. Considering the excessively brief duration of the spark and the very small quantity of matter illuminated, it seems unquestionable that the intrinsic brilliancy is far greater than that of the electric arc, a view fully supported by the results of spectroscopic study.

Various forms of microphone were observed, but the general features characterizing the actions studied were common to them all. As the intensity of the sound acting on the microphone was increased by approaching the organ-pipe to the diaphragm, the motion of the hammer electrode, at first absolutely invisible, was seen gradually to increase, until, when the intensity was very great, the motion was excessive, the anvil electrode being violently pushed aside, and the hammer leaving it on its return motion, so that the circuit was broken at every vibration. At the same time bright sparks were seen between the electrodes. To the ear the simultaneous acoustic changes in the sound transmitted were very striking. The sound of the pipe was distinctly audible, and its quality clear, with motions of the hammer electrode far too slight to be observable. As the sound actuating the telephone became louder, and the excursion of the electrode became visible, the quality continued good, the sound transmitted growing louder; and then, as the excursion increased further, the quality gradually changed, shrill false notes made their appearance, and the sound began to grow harsher, until finally, when breaks appeared in the current, the sound was excessively harsh, and entirely devoid of musical quality. Long before this, however, the characteristic quality of the organ-pipe disappeared.

The following tables will illustrate the results obtained. In making many of the measurements I worked in company with Mr. W. W. Jacques, whose observations were always in substantial accord with my own. A large number of observations have also been made, under my direction, by Messrs. A. W. Jones and F. L. Dame, students in the Laboratory, whose work has been performed with conscientiousness and accuracy.

The microphone used in most of the experiments was one in which the anvil electrode was a "pendulum electrode," suspended by a vertical rod, hinged at the top, and so weighted as to give to it a proper

mass. The desired normal pressure could be obtained by sliding the point of suspension laterally so as slightly to incline the supporting rod, and further, by adding a weight so as to exert a proper leverage, if this was desired. The hammer electrode was pointed and carried by the diaphragm, which was of mica. The sounding pipe was gradually removed from or approached toward the microphone. Care was in all cases taken to keep the wind pressure constant. A single Leclanché cell was ordinarily used, though in a few cases a Grénet cell was employed.

In the various tables, the excursions of the electrodes are in all cases given in fractions of an inch. In designating the material of the electrodes, that of the anvil is stated first.

Tables I. to III. show the results obtained when both electrodes were of carbon; Tables IV. and V., when the anvil electrode was of carbon, the hammer of platinum. Tables VI. to VIII. contain results of observations made with a modified form of transmitter, in which the anvil electrode was also somewhat heavier than in the earlier experiments.

TABLE I.

ELECTRODES, CARBON, CARBON. — *Magnification*, 50 *diameters.*

Excursion.	Character of Sound.
1000×10^{-6} to 700×10^{-6}	Constant breaking and sparks. Electrodes visibly separating.
600×10^{-6}	Constant sparking. Electrodes occasionally seen to separate.
200×10^{-6}	Occasional breaks and sparks.
200×10^{-6}	Scratchy sound, no sparks.
No visible motion.	Sound clear and smooth.

TABLE II.

ELECTRODES, CARBON, CARBON. — *Magnification*, 50 *diameters.*

Excursion.	Character of Sound.
1000×10^{-6} to 600×10^{-6}	Constant sparks; electrodes separating.
300×10^{-6} to 200×10^{-6}	Harsh; occasional breaks and sparks.
150×10^{-6}	Scratchy.
No visible motion.	Clear and smooth.

Whenever the excursion of the hammer was greater than 100×10^{-6} the sound was very scratchy and harsh.

TABLE III.

ELECTRODES, CARBON, CARBON. — *Magnification*, 160 *diameters.*

Excursion.	Character of Sound.
350×10^{-6} to 300×10^{-6}	Loud, noisy, harsh.
250×10^{-6} to 200×10^{-6}	Very scratchy.
120×10^{-6} to 80×10^{-6}	Scratchy, but less than before.
150×10^{-6} to 100×10^{-6}	Scratchy, but with high overtones.

At the lowest amplitude given in Table III. the quality of the sound of the pipe was first heard, but a still less amplitude was necessary for its satisfactory reproduction.

TABLE IV.

ELECTRODES, CARBON, PLATINUM. — *Magnification*, 160 *diameters.*

Excursion.	Character of Sound.
250×10^{-6}	Constant sparks and breaks.
120×10^{-6}	Scratchy; occasional sparks and breaks.
70×10^{-6}	Raspy.
50×10^{-6}	Quality distinct, but high squealing overtones present.
20×10^{-6}	Quality better; fewer high overtones.
Less than 20×10^{-6}	Quality better; some high overtones still audible.

The pipe had to be carried twice as far away from the transmitter as in last measurement, to a distance of eighteen inches, before the quality became really excellent.

TABLE V.

ELECTRODES, CARBON, PLATINUM. — *Magnification*, 150 *diameters.*

Excursion.	Character of Sound.
120×10^{-6}	Very scratchy; occasional sparks.
60×10^{-6}	Squealing, high overtones.
40×10^{-6}	Fair quality; high overtones prominent.
$< 20 \times 10^{-6}$	Good quality; a little rough.
400×10^{-6}	Constant breaking.
60×10^{-6}	Very raspy.
40×10^{-6}	High overtones strong.
20×10^{-6}	Quality good.

TABLE VI.

ELECTRODES, CARBON, PLATINUM. — *Magnification*, 700 *diameters.*

Excursion.	Character of Sound.
200×10^{-6}	Rough, but pitch discernible.
80×10^{-6}	Rough, pitch distinct.
30×10^{-6}	Smooth, with high overtones.
30×10^{-6}	" " "
20×10^{-6}	Quality good; some high overtones.
10×10^{-6}	Good quality.
25×10^{-6}	Smooth, with high overtones.
30×10^{-6} to 40×10^{-6}	High overtones prominent.
80×10^{-6}	Harsh and rough.

TABLE VII.

ELECTRODES, CARBON, PLATINUM. — *Magnification, 940 diameters.*

Excursion.	Character of Sound.
1000×10^{-6} to 800×10^{-6}	Scratchy, with breaks.
100×10^{-6}	Rough, with high overtones.
80×10^{-6}	Wheezy, with high overtones.
40×10^{-6}	Smooth, but with high overtones.
30×10^{-6}	" " "
60×10^{-6}	Rough, with high overtones.
50×10^{-6}	Harsh, with high overtones.
40×10^{-6}	Smoother.
30×10^{-6}	Smoother, with high overtones.
20×10^{-6} to 30×10^{-6}	Good quality, high overtones present.
20×10^{-6}	Good quality.

TABLE VIII.

ELECTRODES, CARBON, PLATINUM. — *Magnification, 940 diameters.*

Excursion.	Character of Sound.
1000×10^{-6}	Harsh and screamy.
2000×10^{-6}	" "
1000×10^{-6}	High overtones heard; no distinct pitch transmitted.
400×10^{-6}	High strident overtones present.
100×10^{-6}	High overtones still present.
60×10^{-6}	Sound wheezy.
30×10^{-6}	Sound smooth.
20×10^{-6} to 10×10^{-6}	Quality good.

The differences in the effects obtained with a microphone in which both electrodes are of carbon, as compared with one in which one of the electrodes is of platinum, are well known to every one who has considered the subject. While with the latter it is more easy to produce an actual break of contact between the electrodes than with the former when the sound is increased, on the other hand the quality is much more satisfactorily reproduced, and does not so rapidly disappear on increasing the loudness. These differences were clearly noticed in the observations. Thus for slight excursions of the hammer electrode the quality of the sound with two carbon electrodes was found to be less satisfactory than when the hammer was of platinum, although in the latter case the point of actual breaking of circuit and sparking was usually reached with a less excursion than in the former one. Evidence of these differences appears in the tables just given, and also in those which follow.

It must be observed, that, while the figures given in the tables show what is the maximum amplitude of vibration of the electrodes con-

sistent with the transmission of quality, they entirely fail to indicate the excessive minuteness of the least excursions which are capable of this result. How minute these sometimes are may be inferred from the following observation.

With a microphone having a somewhat heavy anvil electrode, the organ-pipe was gradually moved away from the diaphragm, and the diminishing range of motion of the electrodes noted in the usual manner. When the pipe was at a distance of three inches the motion of the electrodes was too slight to be visible, although this could have been seen readily with the low magnifying power employed if it had been as great as $\frac{1}{20000}$ in. The pipe, still blown with the same loudness, was then carried farther and farther away. At a distance of thirty-six feet, which was the most distant point from the microphone in the room, the sound of the pipe was still distinctly though faintly audible at the receiver placed in circuit with the microphone, and in a distant apartment.

The results shown in the preceding tables give an idea of the phenomena observable with a microphone of the structure employed. Inasmuch as the primary object of the measurements was to obtain some idea of the actual value of the excursion of the electrode, the mass and normal pressure of the electrodes were not particularly considered, except that they were so adjusted as to give good transmission with moderate loudness of the sound actuating the microphone. But it would of course be expected that the numerical value of the relative excursion of the electrodes corresponding to any given character of sound would vary with the mass of the anvil electrode and with the normal pressure between the electrodes. Two separate sets of observations were made to observe the effects of such variations, by Messrs. Jones and Dame. The latter series was somewhat more complete than the former, besides being carried on with better instrumental appliances, and the results hereafter given are taken chiefly from it.

In all experiments of the nature of those under consideration, it is very difficult to get any fixed standard of quality to which to refer such results as the present. Different observers differ to a certain extent in their estimate of the excellence of the quality reproduced. But the point at which the distinctive quality of the transmitted sound disappears is quite well marked, and a very slightly increased vibration of the hammer electrode causes great harshness to result. For this reason, the name "critical point," originally suggested by Mr. Jones, has been given to this limit, and the excursion corresponding to it has been particularly noted, in tests of the varying effects of mass and pressure.

The transmitter used had its anvil electrode suspended like a pendulum, as before. In order to vary the mass without varying the normal pressure, a horizontal wire was suspended beneath the anvil electrode and rigidly attached to it. The middle point of the wire was vertically beneath the point of suspension of the electrode. The masses added consisted of small copper washers weighing 1.1 grams each. By adding two of these whenever the mass was to be increased, and placing one on each side of the middle point of the horizontal wire, the mass of the electrode was increased, while the normal pressure remained substantially constant. In the experiments whose results are contained in Tables IX. to XIII. the normal pressure was exceedingly small, the electrodes always being kept in very light contact. This condition of things was easily secured by a slight adjustment of the position of the washers. A magnification of 280 diameters was usually employed.

The following results were obtained by the mode of procedure just described.

TABLE IX.

ELECTRODES, CARBON, PLATINUM.

Mass in Grams.	Excursion of Hammer at Critical Point.
4.0	12×10^{-6}
8.1	25×10^{-6}
10.3	37×10^{-6}
12.5	50×10^{-6}
14.7	50×10^{-6}
17.0	50×10^{-6}
19.2	50×10^{-6}
21.4	50×10^{-6}

TABLE X.

ELECTRODES, CARBON, PLATINUM.

Mass in Grams.	Excursion of Hammer at Critical Point.
4.8	12×10^{-6}
5.9	25×10^{-6}
8.1	37×10^{-6}
10.3	37×10^{-6}
12.5	50×10^{-6}
14.7	50×10^{-6}
17.0	50×10^{-6}
19.2	50×10^{-6}

TABLE X. — *Continued.*

Mass in Grams.	Excursion of Hammer at Critical Point.
21.4	50×10^{-6}
25.8	50×10^{-6}
4.5	12×10^{-6}
8.7	25×10^{-6}
10.9	37×10^{-6}
13.1	50×10^{-6}
17.5	50×10^{-6}
21.9	50×10^{-6}

TABLE XI.

ELECTRODES, CARBON, CARBON.

Mass in Grams.	Excursion of Hammer at Critical Point.
8.5	18×10^{-6}
10.7	18×10^{-6}
12.9	25×10^{-6}
15.1	25×10^{-6}
17.3	25×10^{-6}
19.5	37×10^{-6}
21.7	37×10^{-6}
23.9	37×10^{-6}
28.3	37×10^{-6}

TABLE XII.

ELECTRODES, CARBON, CARBON.

Mass in Grams.	Excursion of Hammer at Critical Point.
5.7	12×10^{-6}
8.7	17×10^{-6}
10.9	25×10^{-6}
13.1	37×10^{-6}
15.3	37×10^{-6}
17.5	37×10^{-6}
19.7	37×10^{-6}
5.7	12×10^{-6}
9 1	18×10^{-6}
11 3	37×10^{-6}
15 5	37×10^{-6}
15 7	37×10^{-6}
17 9	37×10^{-6}
20.1	37×10^{-6}

TABLE XIII.

ELECTRODES, PLATINUM, PLATINUM.

Mass in Grams.	Excursion of Hammer at Critical Point.
7.65	12×10^{-6}
9.85	25×10^{-6}
12.05	31×10^{-6}
14.25	37×10^{-6}
16.45	37×10^{-6}
18.65	37×10^{-6}
20.85	37×10^{-6}
7.65	12×10^{-6}
9.85	25×10^{-6}
12 05	37×10^{-6}
14.25	37×10^{-6}
18.65	37×10^{-6}
20.85	37×10^{-6}

An inspection of Tables IX. to XIII. shows that the value of the excursion of the hammer electrode corresponding to the "critical point," and presumably, therefore, the excursion corresponding to any given degree of excellence in the reproduction of quality, at first rises very rapidly as the mass of the anvil electrode is increased, but soon reaches a maximum value, which is not altered by further increase of mass. The rise appears to be less rapid when both electrodes are of carbon, than when one or both of them are of platinum, as may be seen by a comparison of the various tables, or, better still, by plotting the results so as to exhibit them graphically by curves. Also, when both electrodes are of carbon or both of platinum, the maximum and permanent excursion at the critical point is considerably less than when the hammer electrode is of platinum and the anvil of carbon, — a fact which goes to explain the well known excellence of a microphone employing these last materials. Further experiment is desirable, however, before fully accepting this explanation. Furthermore, variations in the surface and shape of the electrodes are likely to modify these values to a certain extent. Thus the carbon electrodes used by Mr. Jones gave the results shown in Tables XIV. and XV. The arrangement of the electrodes was as already described, except that the anvil was slightly inclined, and in the second series a weight of bent wire was added to increase the normal pressure by a small amount.

TABLE XIV.

ELECTRODES, CARBON, CARBON.

Mass in Grams.	Excursion of Hammer at Critical Point.
6.5	36×10^{-6}
8.9	50×10^{-6}
11.3	60×10^{-6}
13.7	60×10^{-6}
16.1	60×10^{-6}
18.5	60×10^{-6}
20.9	60×10^{-6}
23.3	60×10^{-6}
25.7	60×10^{-6}

TABLE XV.

ELECTRODES, CARBON, CARBON.

Mass in Grams.	Excursion of Hammer at Critical Point.
2.9	20×10^{-6}
5.3	30×10^{-6}
7.7	40×10^{-6}
10.1	55×10^{-6}
12.5	60×10^{-6}
14.9	60×10^{-6}
17.3	60×10^{-6}

In connection with the various preceding tables the following results obtained by Mr. Jones will be of interest.

TABLE XVI.

ELECTRODES, CARBON, PLATINUM.

Series.	Magnification.	Excursion for Good Quality.	Excursion at Critical Point.	Excursion for Breaking.
1	400	16×10^{-6}	60×10^{-6}	80×10^{-6}
2	400	20×10^{-6}	$30\text{–}40 \times 10^{-6}$	60×10^{-6}
3	400	30×10^{-6}	50×10^{-6}	80×10^{-6}
4	350	30×10^{-6}	60×10^{-6}	80×10^{-6}
5	700	40×10^{-6}	60×10^{-6}	80×10^{-6}
6	1000	30×10^{-6}	70×10^{-6}	80×10^{-6}

In Series 2 the normal pressure was slightly less than in the others. The excessively high value of 40×10^{-6} at which in Series 5 good quality still persisted, is the highest that has been observed. The hammer electrode had been brought to a sharp point just previously.

I have observed, among the four persons who have employed the method under consideration, that each one has apparently his own standard of what constitutes good quality, and usually adheres quite closely to this in different experiments. The observer last cited generally gave somewhat larger values to the excursion for a given degree of excellence of transmission than I noted in my own observations, apparently from this cause.

A further series of measurements was made in order to ascertain what effect was produced by a variation in the normal pressure between the electrodes.

In order to do this, a wire was caused to project backward from the anvil electrode, and small weights were hung upon the end of it. The pendulum electrode with this projecting wire constituted a bent lever

whose arms were easily measured, thus enabling the normal pressure due to a given weight to be calculated. It was thus obtained very easily, although the method is subject to the objection that there is a certain variation of the mass of the anvil electrode simultaneously with the variation in pressure. The use of a delicate spring instead of the weight would be preferable, but I have not yet found time to carry through observations by this method. The results reached are shown in Tables XVII. to XIX.

TABLE XVII.

ELECTRODES, PLATINUM, PLATINUM.

Normal Pressure in Grams.	Excursion of Hammer at Critical Point.
0.34	8×10^{-6}
0.77	14×10^{-6}
1.20	25×10^{-6}
1.63	37×10^{-6}
2.06	50×10^{-6}
2.50	61×10^{-6}
0.34	10×10^{-6}
0.77	12×10^{-6}
1.20	25×10^{-6}
1.63	37×10^{-6}
2.06	50×10^{-6}
2.50	61×10^{-6}
2.93	61×10^{-6}

TABLE XVIII.

ELECTRODES, CARBON, CARBON.

Normal Pressure in Grams.	Excursion of Hammer at Critical Point.
0.297	8×10^{-6}
0.632	12×10^{-6}
0.990	12×10^{-6}
1.288	12×10^{-6}
1.650	25×10^{-6}
2.150	25×10^{-6}
0.272	8×10^{-6}
0.616	25×10^{-6}
0.960	37×10^{-6}
1.304	37×10^{-6}
1.648	37×10^{-6}
1.990	37×10^{-6}

TABLE XIX.

ELECTRODES, CARBON, PLATINUM.

Normal Pressure in Grams.	Excursion of Hammer at Critical Point.
0.184	8×10^{-6}
0.288	12×10^{-6}
0.432	12×10^{-6}
0.632	12×10^{-6}
0.990	12×10^{-6}
1.288	12×10^{-6}
1.654	Anvil vibrating.
0.104	$>8 \times 10^{-6}$
0.132	$>8 \times 10^{-6}$
0.153	8×10^{-6}
0.184	8×10^{-6}
0.288	12×10^{-6}
0.632	12×10^{-6}
0.990	12×10^{-6}
0.180	8×10^{-6}
0.395	12×10^{-6}
0.620	12×10^{-6}
0.644	12×10^{-6}
0.149	8×10^{-6}

It appears from these results, that the value of the excursion corresponding to the critical point rises with increase of normal pressure, soon attaining a maximum and constant value. The pressure at which this maximum value was reached was greatest when both electrodes

were of platinum, and least when the hammer electrode was of platinum and the anvil of carbon. When both electrodes were of carbon, the value lay between these two extremes. The unexpected result was also reached that the value of the maximum excursion was much greater when both electrodes were of platinum than when both were of carbon. It was least when the hammer was of platinum and the anvil of carbon.

A series of observations was also made upon the microphone of the Blake transmitter. These presented considerable difficulty on account of the construction of the parts of that instrument, and only a low magnification could be used. Three sets of measurements were taken, the first with a very light normal pressure, the second with the ordinary pressure, and the third with a very heavy pressure. With this microphone in its proper condition, both electrodes moved. It was therefore necessary to subtract the excursion of the anvil electrode from that of the hammer, in order to obtain their relative motion. The results reached are given in Table XX. The total excursion of the hammer electrode, and its motion relative to the anvil, are given as nearly as they could be measured.

TABLE XX.

BLAKE TRANSMITTER. — ELECTRODES, CARBON, PLATINUM.

Total Excursion.	Relative Excursion.	Character of Sound.
	a. *Light Normal Pressure.*	
	$<25 \times 10^{-6}$	Good quality, very faint.
50×10^{-6}	25×10^{-6}	" " clear.
100×10^{-6}	50×10^{-6}	Overtones strong.
	b. *Medium Normal Pressure.*	
	$<25 \times 10^{-6}$	Good quality, very faint.
75×10^{-6}	25×10^{-6}	" " clear.
100×10^{-6}	50×10^{-6}	Overtones strong.
	c. *Heavy Normal Pressure.*	
	$<25 \times 10^{-6}$	Good quality, very faint.
100×10^{-6}	25×10^{-6}	" " clear.
150×10^{-6}	50×10^{-6}	Overtones strong.

These results, although of only approximate exactness, are interesting, as they show one cause of the excellence of the Blake transmitter in practice, in that the mode of support of the electrodes allows of very considerable variations in the absolute motions of both of them without material change in their relative motions.

ROGERS LABORATORY OF PHYSICS,
March, 1890.

THE KAOLIN IN BLANDFORD, MASS.

BY WILLIAM O. CROSBY, ASSISTANT PROFESSOR OF MINERALOGY AND LITHOLOGY.

THE existence of a considerable body of white kaolin of a high degree of purity in the town of Blandford, in Hampden County, Massachusetts, has been known for fifteen years. But it is only within the last year that sufficient excavations have been made clearly to reveal the geological relations and probable origin of the kaolin; and even now the full extent of the deposit is very imperfectly known. I am indebted to Mr. George L. Twichell for an opportunity, on two different occasions, to make as thorough a study of the kaolin *in situ* as the excavations would permit. My first examination was made in May, 1889; but since the obvious commercial importance of the deposit gave promise of considerable development during the year, the preparation of this account was purposely delayed until I should be able to make a second visit to Blandford, which was not until April of the present year.

The kaolin has been derived from large veins of a coarse pegmatite, or endogenous granite, containing little mica and consisting chiefly of feldspar, and it is clearly the product of decomposition *in situ* in pre-glacial times, — a genuine remnant of an ancient sedentary soil, which, through some fortunate circumstance, has escaped the powerful erosive action of the great ice-sheet. On account of the relations of the kaolin to glaciation, its topography has a special interest, and will be described in greater detail than would otherwise seem necessary.

Blandford lies on the south side of the Westfield River, well within the border of the crystalline formations, and is a typical example of the hill towns of western Massachusetts. The view, however, from the summit of any of the broadly rounded ridges traversing the town embraces a surprisingly small number of elevations projecting decidedly above the general level. But it is very clear that this region of crystalline and highly disturbed strata, although exhibiting the structure of mountains, is topographically simply a peneplain, having an elevation of fifteen hundred to two thousand feet above the sea, sloping gently eastward toward the Triassic valley, and deeply grooved by the Westfield River and its tributaries. In other words, this is a district which, having been worn down by the agents of erosion nearly to its base level, has been strongly elevated, establishing a new and lower base

level, so long ago that a strongly marked and complex drainage system has since been developed, and yet not so long ago but that considerable remnants of the old peneplain or base-level plain are still preserved in the higher parts of the interstream surfaces.

The village of Blandford Centre is situated on the top, but near the south end, of a prominent ridge which extends in a north-northwest direction for several miles to Green Mountain and the Westfield River, and gives off various spurs or subordinate ridges. The principal part of the village really lies in the faintly marked saddle connecting the main ridge with the north end of a short parallel ridge. The main street of the village runs in a southeasterly direction across the northern end of this subordinate ridge, and makes a slight descent on the east side of it to a nearly level tract or meadow some two hundred feet in width, beyond which the ground rises slightly before descending on the northeast to the valley of Great Brook. The meadow, which appears essentially as a shoulder of the ridge, is merged on the north with the general rounded outline of the ridge; but where it is crossed by the highway it is quite level, and then slopes gently to the southeast, parallel with the road on the northeast, and, in a general way, with the ridge on the southwest, the ridge rising about eighty feet above the meadow. The trend of this ridge is about north-northwest and south-southeast, or almost exactly parallel with the glacial striæ upon its ledges of schist and granite; and it is thus apparent that the elevated meadow, which is the exact location of the kaolin, is not, with reference to glacial movement, in the lee of the ridge, or of any ground rising above its own level, but it was, so far as can be seen, exposed to the full sweep of the ice.

The ridge above the meadow, like the main ridge of which it is a spur, and like all the surrounding country so far as examined, is mainly composed of a distinctly crystalline and typical mica schist. The beds are occasionally garnetiferous, and are quite frequently interstratified with a fine-grained and well laminated hornblende schist. The general strike of the beds is about north and south, and a vertical or very high westerly dip prevails; but both of these elements of the bedding exhibit great irregularities, the strike especially varying widely and rapidly from the normal direction. These local flexures or contortions of the strata are evidently related in many cases to the great veins of coarse granite or pegmatite which traverse the schists. These consist chiefly of coarsely crystalline or cleavable potash feldspar, with usually some muscovite and quartz. The muscovite crystals rarely exceed two or three inches in breadth, and

when larger are likely to be somewhat affected by corrugation and wrinkling. On this account, chiefly, the exploitation some years ago of one of these veins, in which the crystals are larger than usual, for commercial mica and feldspar, proved unprofitable. The quartz is sometimes abundant, especially in the smaller veins, and frequently forms a beautiful graphic granite with the feldspar. Besides these principal or normal constituents of the granite, tourmaline, garnet, cyanite, and more rarely magnetite and beryl, have been observed. The veins vary in width from a few inches to thirty or forty or more feet; and, although running in a general way parallel with the enclosing schists (interbedded veins) they exhibit a great amount of irregularity, frequently breaking across the bedding of the schists, increasing or diminishing rapidly in thickness, and branching or coalescing, so as to enclose considerable masses of schist. The study of the excellent exposures of these highly feldspathic veins on the ridge throws much light on the structure and general relations of the kaolin under the meadow; for the most casual examination shows, as already stated, that the kaolin has resulted from the decay *in situ* of a vein or a connected group of veins of the pegmatite.

At the eastern base of the ridge, about forty feet from the edge of the meadow, and five to ten feet above the level of the latter, excavation has clearly exposed the contact between the mica schist of the ridge and a broad vein of pegmatite, chiefly feldspar, which extends out under the meadow. The contact is parallel with the bedding of the schist, trending about N. 10° W., and dipping west, or under the ridge, at a very high angle. The schist near the contact is pretty well decayed, falling to pieces readily under the pressure of the hand; but it has not been completely kaolinized or reduced to such a plastic earth as may be seen representing original bodies of schist under the meadow. The pegmatite also, near the contact, and generally above the level of the meadow, is imperfectly decomposed, the decay having advanced to the point of general disintegration without marked or extensive kaolinization. But everywhere below the level of the meadow, so far as seen, the decomposition of both the pegmatite and schist is nearly or quite complete; the abundant gray feldspar of the pegmatite being represented by a beautifully soft and white or occasionally pinkish-white kaolin, which is distinctly plastic when kneaded, and appears to be adapted to almost any of the uses of pure kaolin. The following analysis by Dr. L. M. Norton of the Massachusetts Institute of Technology shows that it has approximately the normal composition of kaolin, and the slight excess of silica may be

reasonably regarded as microscopic quartz existing in the original pegmatite.

Silica (SiO_2)	52.03
Alumina (Al_2O_3)	31.76
Water (H_2O)	15.55
Magnesia (MgO)	0.54
Oxide of iron (Fe_2O_3)	Trace.
Lime (CaO)	Trace.
Alkalies (K_2O and Na_2O)	Trace.
	99.88

That the kaolin has been derived from the pegmatite, and that it is a true case of decomposition *in situ*, there can be no doubt. It still shows throughout the structural features of the pegmatite, including even the form of graphic granite when quartz is present. Half-decayed mica crystals and groups of crystals are seen still undisturbed; but the chief interest in this connection attaches to the scattering crystals of tourmaline. These are sometimes a foot in length; but although rotted to a coarse sand or coal-black earth, they are still essentially intact as to form, and present a sharp contrast to the snow-white enclosing kaolin. Small grains and fragments of half-decayed feldspar are sometimes found in the kaolin; and all these facts, as well as its purity, are incompatible with the idea that the kaolin has been washed into its present position. Again, the partially decomposed pegmatite already noted near the contact with the decayed mica schist on the west, and above the level of the meadow, *appears* to pass gradually *downwards* as well as outwards into the kaolin, the decomposition, as stated, being nearly or quite complete for all that part of the pegmatite below the level of the meadow. And, finally, the relations of the kaolin to the schist prove that it is, as a whole, pseudomorphic after one or more veins of pegmatite. The kaolin has been proved to extend east from the west wall of the vein about one hundred feet, or to the middle of the meadow. It is probably not entirely continuous for this breadth, for on the north side of the excavation, which now affords a good transverse section, the kaolin is divided by a sharply defined and vertical mass of thoroughly decomposed mica schist about eight feet wide. This may be regarded as separating two entirely independent veins of pegmatite; or we may think of the veins as uniting immediately to the southward; or the mass of schist may be simply an enclosure in one large vein of pegmatite. In either case it proves conclusively that the decomposed pegmatite or white kaolin is still *in situ*; for although this schist is

now a soft, unctuous and somewhat plastic clay, glistening with the scales of decayed mica, its original bedded structure is still perfectly preserved; and the idea that it might be a bowlder of schist which had been deposited with the kaolin, or subsequently forced into it by the pressure of the ice-sheet, and afterwards decomposed, is pretty well negatived by the fact that it has the normal strike and dip of the unaltered schist in the ridge. Besides, this mass of kaolinized schist and the adjoining white kaolin differ from the rotten schist and pegmatite along the west wall of the vein, which are unquestionably *in situ*, only in being more completely kaolinized owing to their lower positions. A much smaller mass of kaolinized schist appeared to be wholly enclosed in the white kaolin, but the exposure was not entirely satisfactory. The completely decomposed or kaolinized schist is of a distinctly yellowish to brownish color, owing to the peroxidation and hydration of the iron oxide; and the hornblende schist, as we should expect, yields a more deeply colored clay than the mica schist.

Having seen that both the white and colored clays observed here are the products of decomposition *in situ*, being simply kaolinized pegmatite and schist, and hence genuine examples of sedentary or residual soils, their relations to glaciation and the drift deposits next demand attention. Fortunately, the recent excavation is nearly all that could be desired in this respect. It shows, in the first place, that, although the field under which both the white and brown kaolin are found is nearly level, the upper surface of these residual clays is very uneven; for while they come at some points, as along the west side of the field, nearly or quite to the surface, they are in general covered by greatly varying thicknesses of drift. They have been proved by boring and digging to underlie an area at least one hundred feet in breadth (east and west) and three hundred feet in length (north and south); and in a general way the thickness of the drift increases toward the middle of the field, where it is sometimes as much as ten or twelve feet or more. No prospecting for kaolin has yet been done in the east half of the field; but the shape of the ground is certainly favorable to the view that it is underlaid by the sedentary soils, and it is perhaps probable that toward the eastern, as toward the western, border they come nearest to the surface. It is possible, however, that the rocky ridge rising abruptly eighty feet above the western margin of the field, although its trend is parallel with the glacial movement, has in some way afforded more protection to the residual clays at its base than the low ledges bordering the field on the east.

The drift covering the residual clays is chiefly a dark brown or

slightly greenish-brown sandy clay. It is very compact and firm, a true hard-pan, being in this respect a typical till or bowlder clay, which has felt the full weight and prolonged pressure of the ice-sheet. It resembles the till exposed in various road-side sections in this part of Blandford, except that it is less mixed with stones, being of a more uniform character. It is distinguished from the brown residual clay beneath by its greater hardness, the absence of vertical stratification, the occurrence in it of small, hard, worn fragments of quartz, granite, etc., and especially by the imbedded bowlders. Rounded to sub-angular bowlders one to five or six feet in diameter, consisting largely of fine-grained granite and other rocks foreign to this immediate locality, are scattered over the field or protrude from its surface; and quite a number have been removed from the excavation, while others can now be seen imbedded in the hard-pan clay. A sharp line of demarcation usually separates the hard-pan from the underlying residual clays; and in the north end of the excavation it can be seen to pass unchanged from the rotten pegmatite to the rotten schist. That the drift actually covers the residual clays, and that it is the true till or ground moraine, and not any sort of post-glacial deposit, there can be no reasonable doubt.

The most important question remaining is as to whether the decay of the pegmatite and enclosing schist can be proved to date from pre-glacial times. May we not suppose that the till was spread here, as elsewhere in this neighborhood, over the hard, unaltered pegmatite and schist, and that the decomposition of these rocks has subsequently occurred through the action of some purely local cause? The almost impervious hard-pan would certainly tend to prevent the access of atmospheric agents, and thus protect the underlying rocks here, as it does generally, from decay. The fact that the kaolin underlies what may be called a meadow is suggestive of a recent origin; but although the ground is nearly level toward the northwest, it has a decided slope in the opposite direction, and the whole area is well drained, presenting nothing of a boggy or marshy character. The soil is not peaty, and has all been cultivated. Apparently the only basis for a valid argument against the pre-glacial origin of the residual clays is afforded by the fact that a spring issues, except during the driest months, from near the west wall of the decomposed pegmatite vein, at the base of the ridge, and flows down across the field. The water can be seen coming out through the white kaolin, apparently from a considerable depth. It was the attempt to make this water more available for farm uses that led to the discovery of the kaolin.

The spring, however, is local, while the residual clays, as already stated, have been proved to cover a considerable area, and no known facts forbid the belief that they underlie the entire field or meadow. Excavations and borings, made not only near the spring, but at several points from one to two hundred feet away from it in different directions, have proved that the kaolin extends to a depth of at least twenty-five feet, and that at that depth it is still soft and plastic, affording ground for the opinion that its gradual passage downward into hard rock may not occur nearer than fifty or seventy-five feet from the surface. These dimensions indicate a volume of decomposed rock which it seems unreasonable to attribute to the chemical action of one small spring of cold water in post-glacial times. In fact, considering the acid and stable nature of the pegmatite and schist, we can hardly suppose the meteoric waters to possess this degree of potency even under the most favorable conditions, as when aided by an abundance of decaying vegetation, with the resulting organic acids. The schists about Blandford are rarely stained with iron oxide; and there is no indication that they anywhere contain sufficient pyrite to afford by its oxidation enough free sulphuric acid to be of any interest in this connection.

All these considerations in favor of the pre-glacial origin of the kaolin and decayed schist may, however, be waived without conceding the main point, because the fact that tells most strongly in that direction remains to be stated. The excavation shows that, while the till covering the residual clays is so strongly contrasted with them in character, it yet affords evidence that they were in existence when it was formed. Along the contact between the till and kaolin, the latter bears witness to the pressure and friction of the till, having been dragged along and to some extent mixed with the till; and at some points a vertical section shows several irregular layers or streaks of kaolin alternating with the till, or even a detailed interlamination of the two kinds of material. The structure of the till excludes the free action of water in the arrangement of the materials; but it appears probable that water has co-operated in a partial and limited manner with the ice in developing the rude interstratification of the kaolin and till. At any rate, it is clear that the kaolin antedates, in part at least, the accumulation of the till; and since residual deposits or sedentary soils must grow, in general, from the surface downward, it follows that the oldest part of the kaolin, and therefore the part most certainly of pre-glacial origin, is that on which the till immediately rests.

We are thus forced to the consideration of the problem as to how this limited mass of residual clay, a solitary remnant of the wide-spread sedentary soil of pre-glacial times, has escaped obliteration by the ice-sheet. Apparently, the only favoring circumstance besides the ridge on the west is the fact that toward the north-northwest, where the ground begins to descend in that direction, the schist and granite come to the surface in hard, unaltered ledges, affording, perhaps, a partial protection or lee to the kaolin. We may, perhaps, reasonably suppose that the sedentary soil was originally deeper on this level shoulder of the ridge than on the slopes above or below. There are indications of an unusually large development of pegmatite at this point; and the spring is at least suggestive of fractures, or other disturbance, which would permit freer access of the agents of decay. Although the rational explanation of this seeming miracle of the preservation almost intact of a large mass of pre-glacial soil on a hill-top at an elevation of fifteen hundred feet in the midst of a strongly glaciated region, thus obliges us to postulate conditions more favorable in some respects than have been fully proved to exist, I believe one ventures but little in assuming that, owing to structural features which are not now clearly exposed, the decay of the granite and schist extended here to an unusual depth, so that the lower part of the original thickness of residual clays occupies a distinct recess in the hard, unaltered rocks, which have thus protected and preserved it.

Pre-glacial sedentary soils are, of course, not unknown elsewhere in New England; but the Blandford deposit is perhaps unique in the apparent absence of adequate protection from the erosive action of the ice-sheet, if not in the absence, so far as known, of any specially active or efficient agent of decomposition. The well known deposits of white and colored kaolin in Brandon, Monkton, and other towns in Vermont, are covered by drift accumulations, and certainly date from pre-glacial times. They are, however, closely associated not only with limestone, but also with extensive beds of limonite; and since the iron ore has undoubtedly originated in the pyritiferous schists which often accompany the crystalline limestones of western New England, we find in the free sulphuric acid which necessarily results from the oxidation of pyrite a powerful ally of the meteoric waters in the kaolinization of the schists. Besides, the more important of these masses of kaolin enclose considerable bodies of lignite with vegetable fossils which prove it to be of Tertiary age. These white and colored kaolins are thus seen to be, not residual deposits at all, but aqueous or sedimentary accumulations, perhaps of strictly local

origin, comparable, if not synchronous, with the variously colored clays and associated lignite and iron ore of Gay Head on Martha's Vineyard, and showing even more distinctly the disturbing influence of the great ice-sheet.

At other points in the Green Mountain region the iron ores are accompanied by genuine and unquestionable residual clays; and where the vertical position of the strata has favored the downward progress of the meteoric waters, the decay of the rocks is very far from being a merely superficial phenomenon. Thus, I found on visiting the iron mine in West Stockbridge, Mass., several years ago, that the schists enclosing the ore are thoroughly rotted and kaolinized, even in the deepest workings, about three hundred feet below the surface.

Undoubtedly one of the most considerable occurrences of pre-glacial and strictly residual clays yet observed in New England is that penetrated by the Hoosac Tunnel at the western base of the Hoosac Mountains. The decomposed gneiss and schist here have been called the "demoralized" rock, and added greatly to the difficulty and expense of constructing the tunnel. According to the data collected and published by Dr. T. Sterry Hunt, the decomposed rock has a breadth of fully one thousand feet; and the decay is complete to a depth of at least two hundred feet. In other parts of New England veins of pegmatite are found rotted *in situ*, and affording a pure and white kaolin of commercial importance. But although these may perhaps be fairly regarded as remnants of the pre-glacial sedentary soil, the evidence appears to be less perfect and conclusive than in Blandford.

That the lithologic decay in this region during post-glacial time has been almost *nil* is proved by the fact that the removal of the drift exposes everywhere, and on almost all kinds of rocks, a hard, unaltered glaciated surface. I am well persuaded that even the decomposed dikes of diabase which are observed in many parts of New England, and which are a feature of particular interest in the geology of the Boston Basin, simply witness, in general, to the substantial results accomplished by the quiet and unobtrusive atmospheric agents during the long pre-glacial epochs, and owe their preservation to the narrow fissures which they occupy in harder and more durable rocks. This general conclusion appears to be justified by the fact that the great majority of the dikes present hard, glaciated outcrops; and there is only now and then one which was so deeply decomposed that the ice was unable wholly to remove the products of decay. It is undoubtedly true, however, that the more coarsely crystalline diabase

forming the great dikes in Medford, Somerville, etc., like some of the granites, is subject to rapid disintegration by frost action, and that this process is now going on. We may thus readily explain the formation in post-glacial, and even in quite recent times, of the considerable bodies of coarse diabase sand known in this vicinity as Medford gravel and used for walks and carriage roads. This process is mainly mechanical, and such decomposition as is observed to accompany it may be fairly regarded as incidental, and ascribed to the disintegration which enormously increases the surface exposed to the action of the chemical agents; for it is not intended anywhere in this paper to assign greater efficiency to the delithifying agents or chemical erosion in pre-glacial than in post-glacial times, except as this would naturally result from a milder climate. The climatic difference is undoubtedly of vast importance; but nevertheless chemical changes are now in progress, and, given sufficient time or a sufficiently extended surface for their operation, the results must be comparable in magnitude with those observed in unglaciated regions. The superficial oxidation of the drift is one plain indication that chemical changes are now taking place in glaciated regions, while the *general* absence of sedentary soils simply testifies to the shortness of the time since the ice-sheet melted away.

A COURSE OF INSTRUCTION IN RAILROAD MANAGEMENT.

BY C. FRANK ALLEN, Associate Professor of Railroad Engineering.

In our best technical schools the instruction in Civil Engineering necessarily includes the study of many subjects which find their most important applications in connection with railroads.

Such instruction, naturally and properly enough, has commonly been directed to the preparation of the student for the work of constructing railroads, — work peculiarly within the province of the civil engineer; if a young engineer can lay out and construct a railroad of any sort, he is of value; if he can be relied upon to construct safely, he is more useful; while if he can also construct economically, his value to a railroad company is wonderfully enhanced.

Safe construction has for many years been taught with considerable completeness, and at the present time the importance of economy in construction is coming to be more fully appreciated, year by year.

The work of the civil engineer is properly construction; but in railroad work a point has already been reached where it is recognized that the best results cannot be attained without taking carefully into account the effect of construction upon the value of the railroad property with reference especially to operating expenses and to earnings. Economical construction, too, depends not merely upon proper design, but equally upon skilful business management in the execution of the work. From these and other causes, it has resulted that there is demanded from railroad engineers a broader knowledge of many questions connected with the administration and operation of railroads than that which naturally results from instruction in civil engineering, technically considered.

This demand may very properly be met by a course of instruction in Railroad Management, which, however, should not be too strictly limited to the matters above specified, and, in fact, ought not to be confined to the needs of students of civil engineering, or even of young men whose life is to be devoted to railroad work; for railroads are so closely identified with nearly every interest of our country that some knowledge of methods of administration ought to be of interest

and value to every one. If we consider railroads in their various relations to their owners, to their patrons, to their employees, and to the government, we are enabled to gain some idea of the breadth of the subject. Within a few years, the relation of the railroads to the government has assumed more than ordinary importance, and not a little legislation has resulted, having in general for its object, in this country, the protection of the public against any unjust action on the part of the railroads. In fact, the importance of the relation of railroads to the government as a public question has far outstripped the ability of legislators or of citizens to deal wisely with it, and there exists a definite need of growth and development in knowledge of railroad matters among our better educated citizens.

A course of lectures in Railroad Management in a technical school should then be arranged not alone with a view to the better fitting of students for railroad work; it should equally be considered as a branch of liberal education leading to a broader and better citizenship.

It is proper that such a course should presuppose some knowledge of Political Economy, and that it should be treated largely from the economic side. Matters of minor detail should have no place there, except so far as they can be advantageously used in illustration of some principle.

It matters little to us whether a brakeman raises and lowers his lantern, or swings it in a circle, for a signal to an engineer to start or stop a train. There is no matter of principle involved. It might be entirely proper, however, to point out, as a matter of principle, that it is bad management in the case of two neighboring roads that a brakeman on one should raise and lower his lantern, while on the other the brakeman should swing his in a circle, to indicate the same train movement.

At the outset, a brief history of railroads from the beginning may well serve as a foundation upon which to build, and this may profitably be followed by a discussion of the means for internal commerce that existed a century ago, and the progressive steps, including turnpikes and canals, by which railroads became the recognized channels for all long distance inland traffic. The causes and conditions which led to the consolidation of short independent lines into systems for through traffic would naturally follow, and bring us to the consideration of what a railroad is at the present time, as a basis upon which more readily and understandingly to investigate those general railroad questions and problems which should constitute the most important feature of such a course.

In a lecture delivered a few years ago, Charles Francis Adams divided railroad administration into five principal departments: 1. Financial; 2. Construction; 3. Operating; 4. Commercial; 5. Legal; — a classification very convenient for our use.

In the Financial Department, the first question is, properly, whether or not to build the line, and its answer should depend upon the probability of its yielding a profit to its projectors; but this profit may be in the form of an indirect return. A discussion of the nature of a railroad corporation, how formed, how the money is secured, and how spent, will shed much light upon the methods by which profit may be secured; such a discussion should show, for instance, what is the nature of the stockholder's interest in a railroad, and why, in a new and growing country, the proportion of bonds to stock should be large, while in a well settled region, where capital is abundant, the proportion of bonds to stock is likely to be small. Statistics may be given, showing the amount of money invested in railroads; showing, in the case of certain railroads, how much of gross earnings is absorbed in operating expenses, how much goes to interest on bonds, and how much to dividends; giving, also, general data as to the amount of dividends paid, and the proportion of bankruptcies and receiverships. The business conditions which have led to the necessity for corporations rather than partnerships are worthy of attention. Reference should be made to speculation and speculative management, and to stock watering in its several forms, some of which are unobjectionable in point of morality, while others are bad enough. It should also be shown in what way accounts are juggled so that dividends may be declared when none have fairly been earned. These and many other matters form suitable subjects for discussion in considering this department.

The Department of Construction is that in which civil engineering finds its special field. The first question to be decided upon is the route, and it is desirable that the general route shall be settled from an actual examination of the country, previous even to the drawing of the charter. An unfortunate selection of location means, perhaps, steeper grades, lighter trains, and increased expense of operating; it may mean largely increased cost of construction, or a failure to reach important points from which large earnings ought to have been secured, or it may, indeed, involve the company in serious legal complications. An inferior location may handicap the work of each of the other four departments. The attention of the student should be called to such facts, and he should understand that

in construction it is often, though not always, the case, that economy will result from building cheaply at first, and renewing or improving in a more permanent manner at a later date. As an example, bridges of minor importance may be first constructed of wood, and, when renewal is necessary, replaced by structures of stone or iron, which can be hauled by train much more cheaply than by wagon. Embankments, too, may be left low at first, and afterwards raised when steam-shovel and train allow of very cheap handling of material. These matters touch closely, perhaps, upon the technical in civil engineering; but, in addition, there are matters purely of business management, among the most important of which are the letting of contracts and dealing with contractors. There is opportunity for much to be said as to whether a large amount of work should be let under a single contract, or whether in short stretches of a few miles each; then the manner in which specifications shall be drawn, including the basis for estimating the work, may noticeably influence the price of bids, while the personal characteristics of the chief engineer will often affect the cost of work to a considerable extent. The purchase of material, again, offers more than usual temptations to waste. A careful discussion of such questions as those mentioned above is believed to be of the highest interest and importance, and practically outside the matters properly or generally taught as a part of civil engineering.

The Operating Department might, with propriety, include the Commercial; but it is more convenient to separate the two, regarding the operating department as looking after the running of trains, and, for a rough classification, the conduct of those affairs in which expenses are incurred, while the commercial department attends to the securing of business, and deals largely with matters of receipts or revenue. Taking the operating department proper, it was found that, as the business of railroads increased in importance, a superintendent could have personal knowledge of but little of the actual detail work, and as the number of employees became greater, and the duties more varied, the necessity for organization became apparent, and now organization thorough and complete has become a very important feature of railroad operation. A schedule of organization should be given, showing a complete system, from the Board of Directors and the President down to the brakemen, section hands, and day laborers. Much light can be shed upon the operation of a railroad by devoting some attention to the character of the duties of many of the officials whose work is important, though not understood or appreciated by the general public; among these may be mentioned, for example,

the auditor, the purchasing agent, the car recorder, the train despatcher, and, perhaps, the more humble hostlers and track-walkers. The organization and discipline of the working force, the methods of payment of trainmen, the adjustment of lengths of divisions and of train-crew runs, the weight of engine, the capacity of cars, the operation of pusher grades, the best business methods for maintaining good track, the weight and general form of rail in case of renewal, — these and similar questions have bearing upon the cost of operating, sufficient to deserve special comment, or, in some cases, detailed explanation. It is, in short, desirable to touch upon various minor matters not evident to the looker on, which are, nevertheless, important links in the successful operation of a railroad. The value and use of railroad statistics are matters of great importance, and may be properly considered at this time.

The Commercial Department, as has been already stated, is concerned in securing business, and deals with receipts and revenue. No railroad can be managed profitably simply by economy in operating; it is necessary that the securing of revenue should have very careful attention, in order that the gross earnings may be as large as possible. We should know from what sources railroad receipts are derived, and should understand the general principles upon which success in securing them depends; it should be pointed out that in some cases larger returns can be expected to result only from the development of the country traversed by the railroad; in others, the expenditure of effort should be in an attempt to outdo rival lines. There is an excellent opportunity here to show the student the necessity of recognizing that in many cases the prosperity of the railroad is dependent upon the prosperity of the country, and that this prosperity depends largely upon the facilities and encouragment offered by the railroad itself. The interests of the public and of the railroads are largely identical, a fact which the most enlightened railroad managers have long since learned. Either here or elsewhere some mention should be made of the methods adopted for conducting through freight traffic, including some discussion of car "mileage" and "per diem," of classification of rates, of differentials, and various other matters of interest of a similar character.

The Legal Department is not of a character to require that much time should be devoted to a description of its work. It is, however, very desirable that the student should know what constitute the principal duties of a railroad attorney; that the preparation of charters, leases, contracts, and deeds, and the private settlement of claims are

far more important parts of the railroad lawyer's work than the mere trial of cases. The young engineer should, if possible, know something of the legal difficulties to be avoided in the conduct of his own work, and have some idea as to when it will be necessary for him to consult the company's lawyer in order to avoid trouble. While a discussion of the legal department proper does not justify the expenditure of much time, yet an admirable opportunity is presented for giving the student some instruction in such points of law as have a direct bearing upon the conduct of railroad business. Some knowledge of law is desirable for every business man. There are few who do not find it necessary to make contracts of some sort, even if verbal only, and every one ought to understand what features are essential to a valid contract, and, in addition, to know what other provisions good business principles dictate as requisite. Of other law, it is well to dwell somewhat upon such matters as the distinction between common and statute law; between questions of law and questions of fact; the law of common carriers; the law of eminent domain; the essentials of a deed for the transfer of real estate. The instruction in these matters should be brief, and should not involve the technicalities proper for law students; the general principles, however, can and should be set forth in a way to be understood by any student of average intelligence, and sufficiently to enlarge materially his intellectual horizon.

A sufficient foundation should by this time have been established to allow of the profitable consideration of general matters affecting railroad administration. Such for instance, is the principle of competition and combination, applied generally, as well as to railroads; but it should be pointed out that the operation of competition and combination is different in the case of railroads on the one hand, and of general business on the other. The several kinds of monopoly may be referred to and commented upon. The methods adopted by railroads to avoid the evils of competition should be carefully looked into, and this should lead to a discussion of railroad associations and railroad pools. It is desirable to allude to the important changes in business methods which have taken place since railroads were first introduced, what these changes have been, and how they have led to lower rates, — rates low enough to admit of the railroad largely superseding the canal. The methods adopted in making rates, partly in classification, have also led to discriminations of various kinds; and it is upon the matter of discrimination that public interest in railroad affairs has very largely centred. It readily can be, and it should be,

shown that railroad profits are not on the average too great; that railroad charges are not in this country too high; and that the widespread dissatisfaction which has at times been prevalent has been due to discrimination of some sort.

For the evils that have existed many different remedies have been proposed, and a few have been tried. Much that is desired has been reached in some cases by State Railroad Commissions, while in other cases direct legislation, some of it of the "Granger" type, has been resorted to with varying success; various degrees of government supervision and regulation have been attempted, and in foreign countries government control and management have been resorted to; a full discussion of these matters is of extreme importance. In our own country a peculiar difficulty has been experienced, owing to the fact that the separate States could control matters only within their own limits. The outcome has been the passage, within a few years, of the "Interstate Commerce Act," which has naturally and properly attracted public interest more fully than any other railroad matter.

The Interstate Commerce Act should be made the subject of careful and detailed study, and attention should be called to the evils which led to its passage, to the provisions by which those evils were sought to be remedied, and, also, to other provisions adopted for the purpose of rendering the act practically effective. The observed effects of the law in various ways may also be discussed to advantage. In fact, the general discussion of railroad problems can be arranged, very interestingly, in such a way as to lead up to and find its culmination in a thorough discussion of this law in all its bearings. A full appreciation of the points involved cannot well be secured except by a previous understanding of the points developed under the heads of the different departments, including the legal instruction, and very clearly requiring that the student should understand the bearing of competition, discrimination, government control, and all such questions, with reference to this law.

There are also many subjects that may be either treated as isolated subjects, or incorporated in the general discussion, as seems best. The question of taxation is one of some difficulty in any case, and as applied to railroads some unusual methods have been adopted. The ordinary basis, upon real estate and personal property, is not uncommon; but in some States the capital stock forms the basis, while in others the tax is a certain percentage of the earnings; so that, from the variety of views entertained, the subject of taxation of railroads becomes an interesting and important one.

Railroad accidents can be discussed, and statistics given to show their causes and furnish means for judging in what ways we may expect to avoid them, or render them less disastrous when they occur.

It is but a few years since narrow gauge railroads were in great favor, and a discussion of the merits of the narrow gauge, as compared with the standard gauge, may as properly find its place here as in the technical work of railroad engineering.

From time to time, also, there arise questions having temporarily special importance, and requiring on that account more than passing notice. Such, for instance, at the present time in Massachusetts, is the question of the abolition of grade crossings, where every one wants grade crossings done away with provided some one else will pay for it.

The subject of Railroad Management is so broad, that, in arranging a course of lectures upon this subject, there is as great difficulty in knowing what to exclude as in deciding what to include. It is not improper to repeat that the subject should be treated in general from the economic standpoint, and should, it is believed, deal little with details, except for the purpose of illustrating principles, unless perhaps in some cases where it is desirable, or almost necessary, in order to maintain the interest of a class.

The Course of Instruction in Railroad Management above outlined is essentially the one given to students at the Massachusetts Institute of Technology, and has been primarily arranged with a view to the needs of young men whose future will be identified with railroad work. Railroad administration furnishes abundant opportunities for any well educated young man to exercise all the ability which nature and training have placed at his disposal, and a technical training is without doubt the best foundation upon which to build, in anticipation of entering the service of any of our successful railroads. But the importance of the relation of railroads to the State, which is becoming more evident almost day by day, furnishes a reason why a course differing little from the above might with propriety be looked upon as a useful part of a liberal training in colleges where instruction in technical engineering finds no place.

THE DISTRIBUTION OF PHOSPHORUS AND NITROGEN IN THE PRODUCTS OF MODERN MILLING.

BY ELLEN H. RICHARDS, INSTRUCTOR IN SANITARY CHEMISTRY, AND LOTTIE A. BRAGG, S. B.

THE wheat product of the United States is now about 500,000,000 bushels annually, while the production of Indian corn is nearly four times this amount. The wheat is mainly used for human food, while Indian corn is largely fed to animals, so that whatever concerns the best use of wheat concerns the human race. Whole wheat has long been considered one of the few perfect foods containing all the elements necessary to healthful life. When the law of civilization (division of labor) required the large mill to replace the family mortar, it became essential to prepare a flour which would bear transportation from the place of manufacture to the place of use. This requirement could be met only by the removal of the germ and the outer coating of bran, both of which contain certain active ferments, which soon begin to act upon the starch, and so cause the flour to spoil. In order to secure the keeping of the prepared flour for an indefinite time, it was necessary to remove both these substances by a process of sifting or bolting.

After chemical analysis showed the germ to be the richest in nitrogenous substances of any part of the grain, and the bran to be richest in phosphates, it was cleverly argued that the sifted flour had lost its high character as a food, and for the past forty years or more white flour, and white bread made from it, have been the targets at which all food reformers have levelled their arrows.

A measure of opprobrium lingers long after the cause of it has been removed. Although the whole process of flour manufacture has undergone a revolution since 1872, and roller milling has, in the larger establishments at least, taken the place of horizontal stone grinding, and thus the character of the products has been materially changed, the same old arguments still serve, and the same abuse of white bread is found in all books upon food.

It was our intention to compare the products of the two processes as to the per cent of phosphorus and nitrogen in the finer grades of flour; but on consultation with the Flour Inspector of the Boston

Chamber of Commerce, Mr. Towne, we found to our surprise that no mills of any considerable size were using the old stones alone, although a few mills were known to use a combination of both methods. Inquiry in all directions failed to bring to our knowledge even a small country mill turning out the old ground and bolted flour, so complete has been the revolution accomplished in eighteen years.

It was then determined to endeavor to obtain a comparison of flour made from winter wheat and from spring wheat, not only as to composition, but as to distribution of the important elements in nutrition.

We are indebted again to Mr. Towne and to Mr. J. O. Frost for the suggestion of the names of manufacturers whose reputation for progressive spirit and scientific method could be counted upon to respond to our somewhat troublesome request, which was nothing less than a set of samples guaranteed to be fair averages of a lot of wheat and of the products of milling, and also the per cent of each product, in order that we might construct a balance sheet of the phosphorus and nitrogen, and determine its proportionate distribution in the various products.

Well aware that this was a great deal to ask, we preferred our request, with some misgivings, to Messrs. E. T. Archibald & Co., of Dundas, Minnesota, for samples of spring wheat, and to Messrs. E. O. Stannard & Co., of St. Louis, for samples of winter wheat. Both these firms responded with the greatest courtesy and cordiality, and we take great pleasure in acknowledging our indebtedness to them.

Since these samples are from mills not included in the Bulletin of the Department of Agriculture, "An Investigation of the Composition of Wheat and Corn," by Clifford Richardson, the results have an additional value.

The samples came directly from the mills, in little sacks carefully protected, and were put into tightly closed jars as soon as they reached the laboratory.

The samples of St. Louis winter wheat comprised

Whole Wheat,	Low Grade Flour,
Royal Patent Flour,	Middlings,
Extra Fancy Flour,	Bran.

The samples of Minnesota spring wheat comprised

Whole Wheat,	Low Grade Flour,
Patent Flour,	Shorts,
Bakers' Flour.	Bran.

Five determinations were made of the weight of each wheat, the average result being: —

100 grains of	St. Louis Wheat	weighs	2.64 grams.
100 "	Minnesota "	"	2.74 "

The analytical work consisted in the determination of the moisture, nitrogen, and phosphorus in all the samples.

The nitrogen was determined by the Kjeldahl process, as follows: —

The flour was weighed out in a small tube with a loop of platinum wire fused in the end of the glass, so that, when weighed, it could be lowered into a pear-shaped digestion flask without leaving any flour on the neck of the flask. Ten cubic centimeters of strong sulphuric acid were then added, and from 0.25 to 0.3 gram of pure mercury. The flask was placed over a low flame and digested until the solution was colorless. At this stage a few crystals of potassium permanganate were added, until a green color remained. When cool, the contents of the flask were transferred to a round-bottomed flask, using 200 c.c. of water free from ammonia to rinse the flask well. Then 15 c.c. of potassium sulphide solution were added, and lastly 80 c.c. of caustic soda, washing down the neck of the flask with water free from ammonia. The flask was then connected with a proper condenser. Twenty-five cubic centimeters of standardized hydrochloric acid solution were run from the burette into a 250 c.c. receiving flask, and water added, if necessary, to seal the exit tube of the condenser, which was of glass with a bulb at the top. About 200 cubic centimeters were distilled off. Into this was run from a burette enough ammonia solution to make it alkaline, as shown by the yellow color of the solution from the methyl orange, a few drops of which had been added. This alkaline solution was then titrated with standard hydrochloric acid solution, to the exact point of neutralization. The flask was saved, and the color of this used as a standard in titrating the duplicate.

The following are the results obtained by this method.

NITROGEN DETERMINATIONS IN MINNESOTA SPRING WHEAT AND ITS PRODUCTS.

	No. of Determinations.	Nitrogen. Average per cent.
Whole Wheat	5	2.236
Bakers' Flour	7	2.403
Patent Flour	9	2.099
Low Grade Flour	8	2.594
Shorts	8	2.784
Bran	6	2.552

NITROGEN DETERMINATIONS IN ST. LOUIS WINTER WHEAT AND ITS PRODUCTS.

	No. of Determinations.	Nitrogen. Average per cent.
Whole Wheat	6	1.875
Extra Fancy Flour	6	1.787
Royal Patent Flour	7	1.396
Low Grade Flour	5	2.084
Middlings	6	2.738
Bran	6	2.622

To determine the moisture in the wheat and flour several methods were used, with varying success. It was first attempted to dry the samples in tubes made from test tubes, which fitted closely so as to make a nearly air-tight joint. The tubes were placed in a sack which held the covers also, and dried at 110° C., and cooled over calcium chloride. The results were uniform, but in all there was a gain at the third or fourth weighing. After some experiments in drying in an atmosphere of hydrogen, it was decided to adopt an arbitrary method mentioned in one of the Bulletins of the Agricultural Department.

The sample was weighed between watch-glasses, the cover and clip were removed and left in the desiccator, while the flour was put in the hot closet for two hours, then taken out, covered, and cooled in the desiccator for fifteen minutes, weighed, the cover removed, and put back for half an hour. This process was repeated until a constant weight was obtained or the flour showed signs of oxidation. With this conventional method good agreeing results were obtained.

MOISTURE IN ST. LOUIS WHEAT AND PRODUCTS OF MILLING.

	Per cent.
Whole Wheat	12.85
Extra Fancy	12.51
Royal Patent	13.37
Low Grade	11.94
Middlings	11.21
Bran	12.15

MOISTURE IN MINNESOTA WHEAT AND PRODUCTS OF MILLING.

	Per cent.
Whole Wheat	11.09
Patent	12.29
Bakers'	12.14
Low Grade	11.47
Shorts	11.27
Bran	11.23

In the determination of phosphorus in wheat, there has often been doubt expressed whether the phosphorus found in the ash represents the total phosphorus in the wheat. The possibility of losing phosphorus in the incineration naturally suggests itself, but no experiments have been made, as far as we know, to determine whether there was actually any loss under these conditions.

A large number of phosphorus determinations were made on the same sample of flour, by treatment with nitric acid and by fusion with acid potassium sulphate. The results thus obtained were not found to be higher than when the ash of the flour was used, showing that there is no loss in the incineration.

Following are the methods and results of this investigation.

About 2 grams of the flour were heated in a covered beaker with nitric acid of 1.4 sp. gr. until the residue of the evaporation was white; this required many hours, and a large excess of nitric acid. This white residue, dissolved in nitric acid, was precipitated by ammonium molybdate, and the solution of the yellow precipitate treated, as usual, by magnesia mixture.

In the bisulphate process, 2 grams of the flour were intimately mixed with 50 grams of potassium bisulphate, in a large platinum crucible, and a layer of the bisulphate put on top of the mixture. On heating the crucible for about twenty minutes with a good Bunsen burner, the flour is completely burned, and a perfectly white mass remains. This is porous, and disintegrates and dissolves readily in water. To the watery solution was added, when boiling, 20 c.c. acetic acid 1.04 sp. gr., 25 c.c. ammonia 0.96 sp. gr., and 5 c.c. of a solution of ferric chloride containing about 10 milligrams of metallic iron to the cubic centimeter. The precipitate of basic ferric acetate containing all the phosphoric acid was filtered off (after settling), dissolved in nitric acid, and the phosphoric acid precipitated by molybdate solution. A blank analysis gave the phosphoric acid in the reagents used.

The ash of the flour was obtained by igniting in a platinum dish in a muffle, or over a Bunsen burner. The ash was fused with a little sodium carbonate, the silica separated by evaporation with hydrochloric acid, and the phosphoric acid precipitated directly by magnesia mixture, using a little nitric acid to keep up any iron or alumina that might be present.

The results obtained by these processes on a sample of "pastry" flour, source unknown, were as follows: —

	Per cent of Phosphorus.		Per cent of Phosphorus.
Nitric Acid Method,	0.13	Ash,	0.12
" "	0.13	"	0 11
" "	0.13	"	0.11
" "	0.13	"	0.12
Potassium-bisulphate Method,	0.12	"	0.11
" " "	0.11	"	0.12
" " "	0.11		

The following tables give in a condensed form the results of the analyses of the two samples of wheat, and the products derived from them, and a balance sheet deduced therefrom.

ST. LOUIS WINTER WHEAT. MINNESOTA SPRING WHEAT.

	Water.	Phosphorus.	Nitrogen.		Water.	Phosphorus.	Nitrogen.
	Per cent.	Per cent.	Per cent.		Per cent	Per cent.	Per cent.
Whole Wheat	12.85	0 262	1.87	Whole	11.09	0 230	2.24
Royal Patent	13.37	0.051	1.39	Patent	12.29	0.050	2.10
Extra Fancy	12.51	0 100	1 78	Bakers'	12.14	0.091	2.40
Low Grade	11.94	0 100	2 08	Low Grade	11 47	0.192	2 59
Middlings	11 21	0.225	2 73	Shorts	11 27	0.560	2.78
Bran	12.15	0.828	2.62	Bran	11.23	0.830	2.55

BALANCE SHEET.

ST. LOUIS WINTER WHEAT.

	Pounds.		Pounds of Phosphorus.		Pounds of Nitrogen	
Whole Wheat . .	....	100	....	.262	....	1 87
Royal Patent . . .	22.72	....	0 0116	....	0.3158	....
Extra Fancy . . .	39 76	....	0 0397	....	0.7077	....
Low Grade . . .	8 52	....	0.0085	....	0 1772	....
Middlings	3.26	....	0.0073	....	0.0889	....
Bran	24.47	....	0.2066	....	0.6411	....
Total . .	....	98.73	....	.273	....	1 93
			Per cent of total Phosph.		Per cent of total Nitrog.	
In the three grades of Flour	....	....	22.2	....	62.2	....
Middlings and Bran	....	....	77.8	....	37.8	....

MINNESOTA SPRING WHEAT.

	Pounds.		Pounds of Phosphorus.		Pounds of Nitrogen.	
Whole Wheat . .	....	100	....	230	....	2.24
Patent	51.50	....	0.0257	....	1.0815	....
Bakers'	17.50	....	0.0159	....	0.4200	....
Low Grade . . .	3.50	....	0.0070	....	0.0906	....
Shorts	11.00	....	0.0616	....	0.3058	....
Bran	15.50	....	0.1286	....	0.3952	....
Total . .	....	99	....	238	....	2.29
			Per cent of total Phosph.		Per cent of total Nitrog.	
In the three grades of Flour	....	....	20.4	....	69.5	
Shorts and Bran .	....	....	79.6	....	30.5	

This balance sheet defines the position of fine white flour very clearly. It shows that hard spring wheat gives a flour rich in nitrogen, 69.5 per cent of the total being saved in the three grades of flour. Winter wheat does not show quite as well: not only is there more bran, but it is richer in nitrogen. Nevertheless 62.2 per cent of the total nitrogen is saved. There is, therefore, no need to eat whole wheat bread in order to obtain a food rich in nitrogen.

Quite another story is told by a study of the proportion of phosphorus. The two varieties of wheat more nearly approach each other, but the spring wheat loses 79.6 per cent, and the winter wheat 77.8 per cent of its total phosphorus. Here then, is the bone of contention. The fine white flour is sadly deficient in phosphorus, but is the phosphorus which is contained in the bran available for human food? According to the latest experiments of Professors Voit and Rübner in Munich, it would seem that not only is the bran quite indigestible, but that by its irritating action it causes a loss of both nitrogen and carbo-hydrates which would be available in its absence.

It would seem, therefore, fair to conclude that the bread made from fine flour which is most tempting to eyes and palate may, after all, be the one best adapted to the needs and conditions of the human system.

THE LILY DISEASE IN BERMUDA.[1]

BY ALEXANDER L. KEAN.

IN the winter of 1888, through the kindness of General Russell Hastings of Hamilton, Bermuda, I had the opportunity of investigating a disease, epidemic in the lily fields there from March to July. Diseased specimens had previously been shown to several naturalists who had visited Bermuda, and a few of the same had been sent to the United States for examination. None of those who saw these specimens were able, upon a merely cursory examination, to identify the disease; so that the subject still remained open for investigation.

Towards the end of February I sailed for Bermuda, in order to observe personally the phenomena attendant upon the first stages of the disease, which usually begins early in March. A few words on the cultivation of the lily in Bermuda and upon the history of the disease may not be out of place.

The industry is a comparatively new, but prosperous one, being only a few years old. The lily cultivated is the so called Lilium Harrisii, a dwarf variety of Lilium longiflorum. This variety has been propagated from year to year by bulb scales and by a few plants raised yearly from seed. The latter are always more vigorous than those raised from bulb scales; they tend, however, to return to the original longiflorum type. The bulbs, planted in the early autumn, grow throughout the winter months and flower in the spring. They are taken up again early in the summer, and shipped to the United States, where a good market is also found for the flowers in the spring months. The lilies are planted in rows, in small fields; these fields are surrounded by high oleander hedges, which serve to protect the plants from the wind.

So far as I know, the lily disease was first noticed about 1885, since which time it has yearly become more serious, until at present it threatens to do serious damage to the lily crop. It makes its appearance in the spring, when the days begin to grow warm. The marked difference in the temperature between day and night results in a heavy fall of dew, so that even after the sun is high in the heavens large dew-drops may be found upon the lily leaves. A little

[1] Reprinted from the Botanical Gazette, January, 1890.

cool weather, or a few warm, dry days free from dew, will check the activity of the disease, while a warm, damp day will cause it to spread with great rapidity. The disease first appears as a minute orange-colored spot upon a leaf or flower, usually on the upper side of the leaf. Figure 1 (*a*) shows an early stage. The spot gradually increases in size, and finally spreads throughout the whole leaf. In this way whole plants may be killed, and only the stalks left standing. It is not unusual to find several of these spots on the same leaf, as in Figure 1. If, for any reason, the progress of the disease is checked, these become dry, leathery patches of a buff color.

It was thought possible, by some of those who had seen the diseased specimens, that the spots might be due to the stings of insects. Though at first this did not seem unlikely, a closer acquaintance with the disease rendered it highly improbable.

Sections of the spots examined under the microscope revealed nothing more than that in the diseased area the structure of the leaf had collapsed; while along the edges of the spot the cells were somewhat swollen and the cell walls thickened. The cell contents were slightly shrunken, and contained a number of bright dots, which did not stain with the ordinary reagents. In a few cases where specimens were teased, an occasional hypha of a fungus was found, which, however, might easily have come from external contamination. In a slightly more advanced stage of the disease, hyphæ of a fungus might be found ramifying through the soft decaying tissues, the gonidiophores appearing on the surface (Fig. 2).

This fungus invariably appears in advanced stages of the disease; moreover, it is always the first to appear. Other fungi, such as Macrosporium, Eurotium, and Penicillium, also appear on the decaying leaves, but this fungus invariably precedes all others. At the same time, as it could never be found until the tissue had become pretty thoroughly rotten, it seemed at first sight to be rather a consequence than a cause of the disease.

Early in April I was obliged to return to the United States, and for a time my investigations were interrupted, to be resumed again in the summer, when I had specimens forwarded to me from Bermuda. It had seemed possible that the disease might be due to bacteria, on account of its peculiarly rapid growth and its susceptibility to climatic conditions, as well as for other reasons. Accordingly a series of cultures in nutrient gelatine and agar-agar was begun. By this method two distinct species of bacteria were isolated from the spots, either of which might have been the cause of the disease.

Inoculations were attempted, both in the laboratory and in the garden, all of which failed to produce the disease; but as the American climate is dry, and therefore unfavorable, it was thought that the bacteria had not been given a sufficient trial, and so it was decided to let the whole matter lie over until the next winter, when inoculation experiments could be made in Bermuda.

Shortly before visiting Bermuda for the second time, my attention was again directed to the fungus as a cause of the disease by the appearance of Professor Marshall Ward's description of a similar disease caused by a fungus growing upon the Lilium candidum in England. Upon my arrival in Bermuda, in February, 1889, I renewed my efforts at inoculation with bacteria, but in every case without success. I also placed a number of plants in glass jars which contained fluid cultures, to see if by any chance the action of bacteria upon the bulb might not have something to do with the epidemic. These plants, however, remained remarkably healthy, as did also a number of plants from which I removed the bulbs, placing the stalks in fluid cultures, so that the bacteria had direct access to the tissues of the plants. The weather was extremely unfavorable, so that, although I saw numerous small spots, I met with none large enough to show gonidiophores of the fungus. I was therefore unable to complete my experiments satisfactorily in Bermuda, as I could not even get a culture of the fungus as a starting point. Since my return, however, I have had specimens sent to me and have been able to carry on a series of experiments in the laboratory, by means of which I have at length ascertained definitely the cause of the disease. Meanwhile I have repeatedly tried to inoculate plants in the laboratory with bacteria, all of which attempts have been signal failures, as have been, likewise, all attempts to produce the disease by fungus spores sown in a drop of Irish moss which had been placed upon a leaf. In the latter case it was found impossible, without resort to artificial means, to keep the air of the room moist enough to prevent the drop of Irish moss from drying up soon after it was put upon the leaf. I readily succeeded, however, in getting numerous Irish moss cultures of the fungus, and I have also grown it in water, both ordinary and sterilized, and in hanging drops as well as in drops merely placed upon a slide.

In my next experiments, leaves freshly picked from the plant were used and kept fresh in a beaker of water, which was placed under a bell glass where there was a second beaker of steaming water, which was occasionally renewed so that the atmosphere was kept warm and moist, thus making the conditions highly favorable. Bacteriological

experiments under these conditions failed as had all previous experiments. On the other hand, experiments with the fungus were entirely successful. If a drop of Irish moss placed on the surface of a leaf is inoculated with spores of the fungus mentioned above, a disease spot will appear in about two days, unless in some way the drop has dried up, or been otherwise disturbed. If the drop has simply dried up, it is only necessary to moisten it again and the spot will soon appear. About the third or fourth day the gonidiophores appear and soon bear compact bunches of gonidia, the whole forming a downy growth on both sides of the leaf. It makes no difference whether the original spores are sown in a drop on the upper or the under side of the leaf. In either case the disease is produced. I have substituted drops of water for the Irish moss as a culture medium with equal success, and, to exclude contamination, drops of sterilized water were used, with which I also obtained excellent results.

To prove beyond all doubt that the fungus is the cause of the disease, the following experiment was made. Into a tube of sterilized nutrient gelatine some fungus spores were introduced on the tip of a sterilized needle. The contents of the tube were then "plated" according to Koch's method. The fungus grows luxuriantly in this medium, and can thus be obtained free from bacteria or other fungi. Spores are less quickly produced on the rich gelatine than on the comparatively poor Irish moss. Inoculations made with spores from a pure culture of this kind readily produced the characteristic spots, while blank control experiments showed nothing whatever.

The fungus appeared to me to be identical with the Botrytis recently described by Professor Marshall Ward[1] as growing upon the Lilium candidum in England, and from a specimen sent to him Professor Ward has identified it as the same. So far as I know, the Lilium candidum is exempt from the disease in Bermuda, but this species is only occasionally grown there, and then with only a few together. Under similar circumstances the longiflorum also is generally exempt, the plants in the fields alone suffering. The true longiflorum is not considered to be as susceptible to the disease as the Harrisii. Since Professor Ward has described the Botrytis and its method of growth in so complete a manner, it would be superfluous for me to enter upon a detailed description of it. I shall, therefore, give but a brief summary of the main facts in its life history, as I have observed them.

[1] A Lily Disease, by H. Marshall Ward, M. A., F. R. S., F. L. S. Annals of Botany, Vol. II. No. 7, November, 1888.

The gonidia are ovoid in shape, about 0.02 mm. long and about 0.015 mm. broad. They are at first colorless, but when mature are light brown, the whole gonidiophore passing through the same change of color. The gonidia are attached to the gonidiophores by means of slender sterigmata. These sterigmata are frequently found still adhering to the spore after it is entirely separated from the gonidiophore upon which it grew. While the normal shape of the spore is ovoid, spores are often found of different shapes, as those depicted in Figure 7. These forms, however, seem to belong to the period of germination, being simply conditions which the gonidia assume in the process of swelling. The spores are almost always ovoid, although in some old bunches they may sometimes be elongated, and even divided by a partition. In germination, the gonidium swells, the protoplasm becoming quite dense. Soon after, the wall begins to bulge out in several places, from which hyphæ are eventually protruded. One of these is generally more vigorous than the others, and grows much more rapidly, producing a complicated mycelium, while the other hyphæ are scarcely more than buds. The protoplasm of the rapidly growing tips of the hyphæ is quite dense, while that in the older hyphæ is clear and much vacuolated. Two remarkable features of the mycelium described by Professor Ward I have also observed. These are, first, organs of attachment, consisting of thick cone-shaped tufts (Fig. 5), developed upon the hyphæ when they come in contact with any foreign body which they are not able to penetrate. The second peculiarity is the development of cross branches between contiguous hyphæ, thus forming a network in the mycelium (Fig. 6). The gonidiophores are, on the leaves, as a rule, about one millimeter in height. When they reach this height, their tips begin to swell and numerous small peg-like processes appear upon the heads thus formed. These are the sterigmata, and on each of these a gonidium is formed (Fig. 3). There are seldom fewer than four gonidia in a head and they usually bear a much larger number. With perhaps thirty of these heads to the square millimeter, which is about the average number on an ordinary diseased leaf, it is not strange that, under favorable conditions, the disease should spread from leaf to leaf and from plant to plant with surprising rapidity. If the growth of the fungus is unchecked, the erect hyphæ may bud out just below the gonidiophores, and, after growing a short distance, produce another bunch of gonidia, and so on, thus forming a series such as is shown in Figure 4.

In this disease the fungus does not grow in the sound tissues of the host, extracting nourishment from them, but the spores, germinating

upon the surface of the leaf, in some way macerate it. According to Professor Ward, this is done by means of a ferment secreted by the tips of the hyphæ. I have not as yet been able to experiment upon this ferment; but I have seen nothing to invalidate his view. The cell walls having been softened, the hyphæ pass through them, ramifying amongst the decaying tissues of the leaf.

The principal conclusions arrived at in this paper are the following: —

1. A disease, hitherto unexplained, and threatening to become a serious epidemic in the lily fields of Bermuda, has been assigned to a definite cause.

2. There is no evidence that the blight is due to the stings of insects.

3. There is no evidence that the disease is caused by bacteria.

4. There is strong evidence that the disease is caused by a fungus, growing upon and within the leaves or flowers.

5. The fungus which causes the Bermuda lily disease is identical with the Botrytis recently described as the cause of a similar disease in England.

In making the investigation just described, it was hoped, not only to discover the cause of the disease, but also to suggest remedial measures. In this connection I may remark that the fungus itself seems to be delicate, so that its growth might probably be checked by almost any of the poisons used in the treatment of plant diseases, were it not for the difficulty of applying these effectively. Either a powder sprinkled on the leaves, or a liquid sprayed upon them, would here be ineffective, for the moisture would soon remove them, leaving the fungus free to grow. If, however, the fungus were present at the time of the application of the remedy, that part of it growing outside of the leaf might be killed, although that would not prevent the mycelium within the leaf from continuing to grow.

I have, however, observed that plants growing under the shelter of the oleander hedges are remarkably exempt from the disease. This I attributed at first to the drippings from the leaves, which I thought might partake of the acrid character of the sap of the oleander. I am inclined to believe, however, that it is rather because the lilies under the hedges are remarkably free from moisture. In fact, the overhanging hedge collects most of the dew, so that the dew-drops do not readily gather upon the leaves of the lily plants, and thus, perhaps, no opportunity is given for the growth of the fungus.

It might, therefore, be found feasible to grow, in alternate rows with the lilies, some other crop which, being higher and having more spreading foliage, should keep off the dew. A row of stakes with branches wattled among them might serve the same purpose and check the virulence, even if it did not entirely prevent the recurrence, of the disease.

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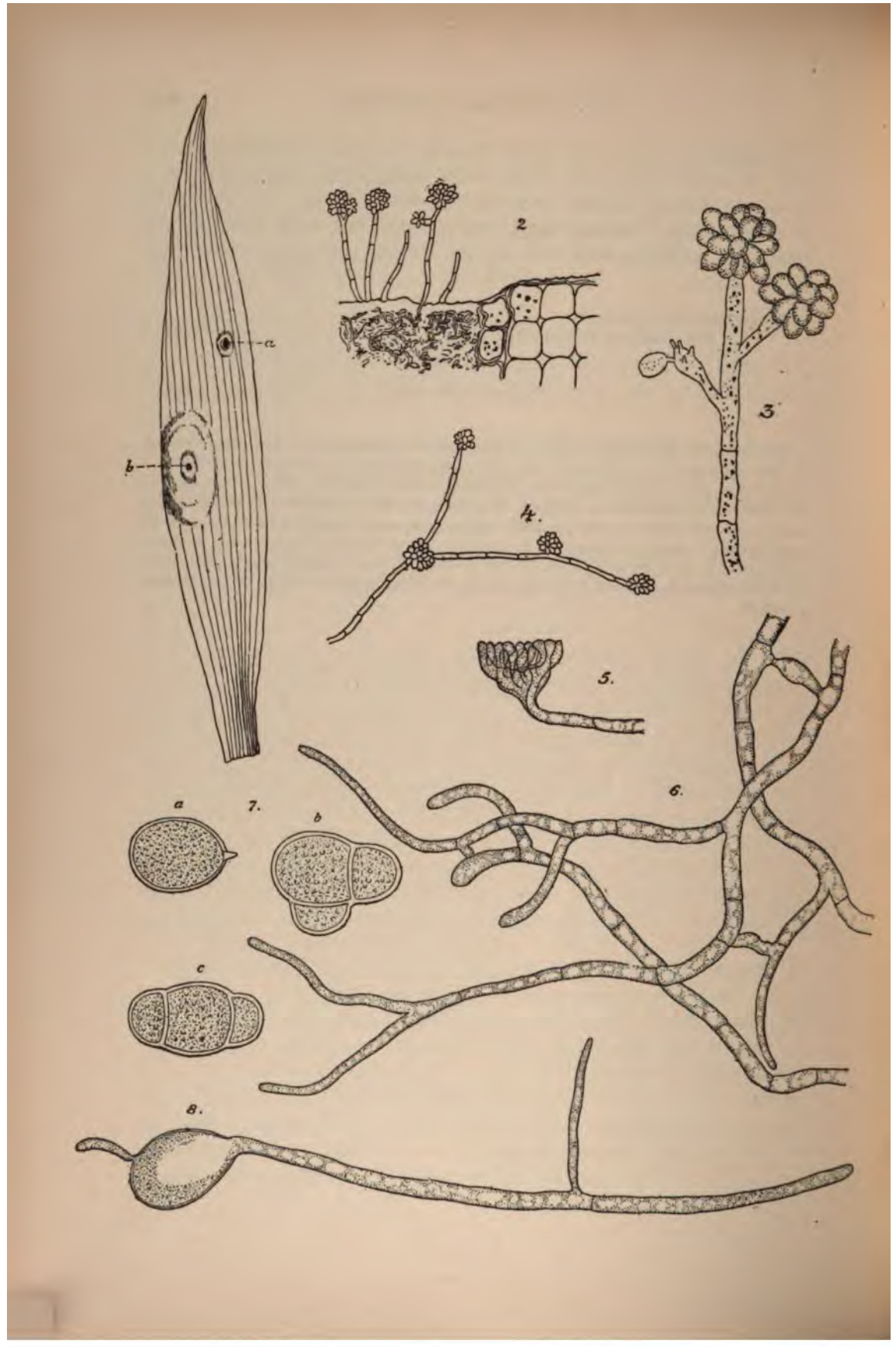
EXPLANATION OF PLATE. — Fig. 1. Upper side of a leaf, with two spots upon it, showing different stages of the disease; (*a*) an early, and (*b*) a more advanced stage. — Fig. 2. A section through a diseased spot when the disease is far advanced. The hyphæ occupy all the tissue in the diseased area. — Fig. 3. A gonidiophore with its gonidia, showing the sterigmata and the attachment of the gonidia. — Fig. 4. A series of bunches of gonidia on an old gonidiophore. — Fig. 5. An organ of attachment. (After Marshall Ward.) — Fig. 6. Part of a mycelium showing cross branches. — Fig. 7. Gonidia; (*a*) an ordinary gonidium; (*b*) and (*c*) forms frequently assumed by gonidia shortly before germination. — Fig. 8. A sprouting gonidium.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THE DRY ASSAY OF TIN ORES.[1]

PART II.

BY HEINRICH O. HOFMAN, ASSISTANT PROFESSOR OF MINING AND METALLURGY.

Class A (b).

THIS class embraces the bronze assay of Winkler, and the different iron assays.

4. Winkler's method[2] is as follows: 5 grams of ore are mixed with 5 grams of cupric oxide and charged in a clay crucible; on top are placed 15 grams of black flux with 1.25 grams of borax glass, then a salt cover, and finally a large piece of charcoal. The crucible is heated in a muffle or a pot-furnace, after boiling has ceased, for three quarters of an hour to an hour at a bright red heat, finishing the assay at almost a white heat. The result is a white, brittle, tin-copper alloy. With every tin assay, an assay of 5 grams of cupric oxide with the same charge is made, and the weight of the resulting button of copper, subtracted from the bronze button, gives the weight of the tin contained in the ore.

This assay is really a modification of the German method. Its advantage is that no prills of tin (or, rather, particles of bronze) are found suspended in the slag. All the metallic oxides that have been reduced are found collected in one button. It is claimed that more tin is recovered by this method than by the regular German method, and that the buttons do not differ more than from 0.4 to 0.5 per cent. If this could be substantiated, the method would be a serious rival to the cyanide assay.

In the experiments carried out the directions given above were closely followed, only the black flux substitute (2 potassium carbonate and 1 flour) was used, as in the regular German method, instead of the black flux proper.

The charges were readily fusible, liquid, and showed a smooth surface when cool. The crucibles were not attacked, and no particles of bronze were discernible in the slags. As in the German assay, the

[1] Continued from Vol. III. No. 2, May, 1890.

[2] Berg- und Hüttenmännische Zeitung, 1864, p. 17; also Kerl, *op. cit.*, p. 483, and Balling, Die Probirkunde, p. 391.

upper salt slag and the lower borax slag have to be distinguished. The salt slag separates readily from the lower slag, has a crystalline structure, is brittle, has a pearly lustre, and is opaque. In the centre of every salt slag is a small cavity, into which white, fern-like crystals protrude. The white base of the crystals is encircled by a green-tinged shell, and this by the bulk of the slag, which is of a lighter purple color than that of the German assay. The reason is probably that less carbon is present in the charge, no charcoal having been added. The vitreous borax slag is hard, opaque, and greenish black. The oval bronze button is smooth, crystalline, brittle, white, and the more rounded copper button has thread-like lines of crystals on the surface, and is of a beautiful rose-color. Both separate well from their slags.

TABLE XVIII.

No. of Assay.	Resulting Bronze.	Resulting Copper.	Resulting Tin.	
	Grams.	Grams	Grams.	Per cent.
95	7.200	3.825	3.375	67.50
96	7.170	3.805	3.365	67.30
97	7.110	3.785	3.325	66.50
98	7.105	3.785	3.320	66.40
99	7.040	3.820	3.220	64.40
100	7.020	3.770	3.250	65.00
101	7.000	3.770	3.230	64.60
Average	7.092	3.794	3.298	65.96
102	7.185	3.920	3.265	65.30
103	7.140	3.920	3.220	64.40
Average	7.162	3.920	2.2425	64.85

Table XVIII. shows that too much has been claimed for this method, both as to output of tin and accuracy of results. The total average, Nos. 95 to 101 (65.96 per cent tin), is 1.88 per cent too low, and while in some cases excellent results are obtained (Nos. 95

and 96), in others, under the same conditions, an assay can be 3.24 per cent too low. It was thought that perhaps, by increasing the amount of reducing agent, better results could be obtained. Thus, 1 gram of charcoal was mixed with the 5 grams of ore and 5 grams of cupric oxide, and then the usual charge given. The results (Nos. 102 and 103) show only an increase of weight in the copper recovered. Chalk-lined crucibles were tried, as in the German method. But, while no essential particular could be observed in which the assay differed from that in the naked crucible, the results given in Table XIX. show that the discrepancy in the resulting weights of tin is enormous, while the output of copper is higher, and very even.

TABLE XIX.

No. of Assay	Resulting Bronze.	Resulting Copper.	Resulting Tin.	
	Grams.	Grams.	Grams.	Per cent.
104	6.850	3.815	3.035	60.70
105	6.660	3.815	2.845	56.90
106	6.760	3.810	2.950	59.00
107	6.250	3.810	2.440	48.80

As a modification, therefore, the chalk-lined crucible must here also be left out of consideration.

The reason that the results with the Winkler method vary so much is that the presence of copper adds a new difficulty to the assay. This can be seen from the tabulated results obtained in reducing cupric oxide for the tin assays carried out above.

In all the assays, precipitated, chemically pure cupric oxide was used. If reduced completely, 5 grams of cupric oxide ought to give 3.993 grams of copper. In the naked crucible we obtain as an average 3.801 grams, and a discrepancy of 0.055 gram between the highest and lowest results, which, if expressed in percentage, gives 1.1 per cent difference. In the chalk-lined crucible there is a better output in copper; if extra reducing agents be added a still better result is obtained, even in the naked crucible, but not a correspondingly better reduction of stannic oxide. It would seem, therefore, that the amount of tin reduced will vary according to the amount of copper carried off in the slag.

TABLE XX.

Grams of Copper resulting in Naked Crucible with Regular Charge.	Grams of Copper resulting in Naked Crucible with Regular Charge and one Gram Charcoal.	Grams of Copper resulting in Chalk-lined Crucible with Regular Charge.
3.825	3.920	3.815
3.820	....	3.810
3.805	....	....
3.785	....	....
3.770	....	....
Aver. 3.801	3 920	3.8125

In the assays with potassium cyanide it was seen that the presence of sulphates had a very bad effect on the result. As copper has a very great affinity for sulphur, the results of this bronze assay may also have been influenced by it. Several assays were made, omitting the salt and increasing the amount of potassium carbonate. The weight of the copper buttons was slightly higher, e. g. 3.850 grams; but the reduction of the tin was very imperfect, as it did not all alloy with the copper and collect in one button of bronze; prills of bright, malleable tin were found in the slag, showing that, to obtain a slag of sufficient fluidity for all reduced metal to collect in one button, the addition of salt is necessary. By reference to the German assay it will be seen that the presence of salt containing sulphates does not produce the same bad effect on tin as in the cyanide assay.

The Winkler method is given in a modified way by Ricketts:[1] ore, 10 grams; cupric oxide, 10 grams; black flux substitute, 30 grams; argol, 2 grams; borax glass, 5 grams, — are well mixed, charged in a chalk-lined crucible, a salt cover and charcoal are added, and the charge heated gradually to a white heat for an hour. He advises separating tin and copper in the wet way. In this modified method it will be seen that an additional reducing agent, argol, has been added, and that flux and ore are intimately mixed. Winkler makes a point of not mixing ore and flux, as he wishes to avoid the strong

[1] Notes on Assaying, 1886, p. 89; quoting from Mitchell, Manual of Assaying, p. 411 (probably fourth edition, as it has been omitted in the fifth edition of 1881).

boiling of the charge, whereby particles of alloy might adhere to the sides of the crucible and escape being collected in the button.

Three bronze assays were carried out and one copper assay, giving the following results.

TABLE XXI.

No. of Assay.	Resulting Bronze.	Resulting Copper.	Resulting Tin.	
	Grams.	Grams.	Grams.	Per cent.
108	13.685	7.360	6.325	63.25
109	13.660	7.360	6.300	63.00
110	13.630	7.360	6.270	62.70
Average	13.658	7.360	6.298	62.98

They show no advantage over Winkler's original method. Less tin is recovered and more copper is scorified, and a treatment of the alloy in the wet way would be apparently a loss of time.

5. *Iron Assays.* — Two sets of assays were made, the one recommended by Ricketts,[1] for tin ores containing silica only, the other by Mitchell,[2] for ores containing silica and for tin slags.

The method is based on the fact discovered by Berthier,[3] and already alluded to, namely, that iron completely precipitates tin from its combination with silica as metal, if added in sufficient quantity; if not, the stannic oxide is reduced only to stannous oxide, the iron itself being at the same time oxidized to ferrous oxide. Any excess of iron will alloy with the resulting metallic tin.

(1.) Ricketts gives the formula: ore, 10 grams; hematite, 3 to 8 grams; potassium cyanide, 40 grams; and says: "Mix and charge in a charcoal-lined crucible, cover with cyanide, and then with charcoal, lute and heat strongly from one half to one hour, remove, tap carefully, cool, and break. If the tin be in small buttons, collect by washing with water to separate the charcoal, dry, and weigh. Treat the button as an alloy of tin and iron."

The result of the experiment was a porous, grayish-black slag, through which small buttons of alloy were finely disseminated, and

[1] Notes on Assaying, 1886, p. 89.
[2] Manual of Assaying, 1881, p. 483.
[3] Traité des Essais, 1847, Vol II pp. 459, 460, 484.

in the bottom of the crucible a button. Button and larger prills were picked out, the crucible lining crushed, the pulp screened, the scales and the siftings obtained from panning the pulp weighed with button and larger prills. The weights of the alloys were 8.355 and 8.320 grams, which shows a close agreement, if the amount of iron is the same in both cases. This assay was not further pursued, as there are more simple methods for obtaining accurate results. The silica of ores "containing silica only" can be treated in a much more simple way. It can be removed by washing, and the pure black tin assayed with potassium cyanide, or, if too great a loss of cassiterite is feared, the silica can be removed by hydrofluoric acid, and the resulting pure black tin then assayed. This method of assay has therefore little value for the assayer, except that it substantiates an interesting fact, namely, that iron carries down all the tin.

(2.) Mitchell[1] mixes 400 grains of ore with 200 grains of ferric oxide, 100 grains of fluorspar, and 100 grains of charcoal powder, charges the mixture in a covered crucible, keeps it at a dull red heat for half an hour, and then at a white heat for another half-hour.

In the experiments the above quantities were reduced to a basis of 10 grams of ore. These resulted in a hard, rough, bluish-black shell, with bronze-colored stains. The inside was filled with fine charcoal, no button or any prills being visible. Apparently the ore and the hematite had united to form a slag. The method was not further investigated. If tin slags are to be assayed, it is a simple matter to decompose the finely pulverized slag with sodium or potassium disulphate and then treat with acidulated water, when stannic oxide and silica will be left. The tin in this mixture can then be readily determined.

(3.) One more experiment remains to be mentioned here, i. e. the fusing of black tin with potassium ferrocyanide and potassium cyanide. Bloxam[2] states that by using this reagent all the tin can be obtained as an alloy. The idea that led to the experiments was, that, if a weighed amount of dried ferrocyanide were fused with the ore, the buttons obtained from a number of assays would all have the same weight. If all the iron set free by the decomposition of the ferrocyanide alloyed with the tin, the percentage of tin could be readily calculated.

In the experiments, 5 grams of ore were mixed with 5 grams of dried potassium ferrocyanide and 5 grams of potassium cyanide. For

[1] Manual of Assaying, 1881, p. 484.

[2] Journal of the Chemical Society, New Series, Vol. III., 1865.

the bottom of the crucible and for the cover 5 grams of potassium cyanide were reserved. The results of the experiments were negative. The alloy was partly disseminated through the lower gray slag, which was porous and tough, and had partly collected in a button. The weights of several 5-gram assays did not agree within 1 gram, showing that all the iron of the ferrocyanide had not united with the tin, but that part had remained suspended in the lower slag.

Class B.

The second group of assays comprises the Cornish methods, which are only for determining the tin obtained by treating the ores on a large scale. That the results of such assays must be wrong is conceded. How closely they may agree when repeated in the same way by the same person is not stated.

6. Mitchell[1] and Ricketts[2] give the following method "for pure tin oxide": 400 grains (Mitchell) or 10 grams (Ricketts) are placed in a charcoal-lined crucible, to which a cover is luted. The crucible is heated gradually for twenty-five minutes, finishing the operation at a white heat.

For the experiments, 25 grams of ore were treated as described. There resulted, in the bottom of the crucible, a dull button of tin, while on the walls and bottom were numberless bright prills of tin. Button and prills were free from iron. The charcoal lining was pulverized and made to pass a 60-mesh sieve. The button, the scales on the sieve, and the siftings resulting from the pulp that had been panned were weighed together. The weights of two assays were: —

18.120 grams = 72.5 per cent tin.
16.150 " = 64.6 " "

The chemical analysis shows only 67.84 per cent of tin. The reason for this strange result is to be sought in the panning of the fine pulp. When all the charcoal has been washed off there remains in the pan a heavy, black residue. It is almost impossible to see what part of this is fine tin and what heavy slag, as they scarcely differ in appearance. It is certain that with the result 72.5 per cent slag still remained with the tin, and it is not at all sure that it was all removed with the result 64.7 per cent. Perhaps after long acquaintance with uniform ores some approximately reliable estimates might be obtained, but as far as exact results go the method appears to the writer a very doubtful one.

[1] *Op. cit*, p. 480. [2] *Op. cit.*, p. 88.

7. Another method is given by Mitchell[1]: 2 ounces of ore are mixed with a small quantity of culm and projected into a red-hot, naked crucible; some fluorspar is added if necessary, and after a quarter of an hour's fusion the tin is poured. The prills in the slag are recovered by pounding and vanning.

In the experiments, 50 grams of ore were mixed with 12 grams of finely pulverized anthracite, shot into the red-hot crucible and heated for half an hour, a little fluorspar having been added. No fusion was apparent. The crucible was removed, and broken when cold. It contained a black, porous, fritted mass, full of prills of tin, and on the bottom a button. The entire lower part of the crucible was crushed, screened, and panned, and all the recovered tin weighed together. The weights were: —

34.10 grams = 68.20 per cent tin.
27.38 " = 54.70 " "

The same difficulty as above in separating the heavy, black slag from the fine tin causes the irregularity.

8. In connection with the Cornish methods it may be mentioned here that it has been the writer's custom, in melting out metallic tin from different grades of black tin, to mix the black tin with 5 per cent of lime, 5 per cent of fluorspar, and 12 to 15 per cent of charcoal or anthracite, the latter requiring a higher temperature but giving cleaner slags. With pure concentrates obtained from ore occurring in quartz no special difficulties have been encountered, so that the entire contents of a plumbago crucible weighing about 10 pounds can be readily poured. With impure concentrates the results of such fusions have always been somewhat pasty slags, and additions of lime or fluorspar were never of much avail. The only remedy has been to add salt, which makes the slags less pasty. The main point in all these fusions is always to have the crucible nearly filled. If the first charge has begun to fuse, a second one is added. Such a fusion takes from one to two hours. Unfortunately, no records have been kept of the amount of white tin contained in the black, which would give the amounts saved.

In reviewing the preceding experiments, it is striking that so many different methods and modifications should still be recommended when a few simple ones are quite sufficient. The experiments made show clearly that there are only two methods giving entirely satisfactory results. These are the German method, as exemplified by assays

[1] *Op. cit.*, pp 480 and 481.

Nos. 1 to 6, and the method of fusion with potassium cyanide, as from assays Nos. 35 to 38; or, still better, if sufficient material is available, Nos. 39 to 42.

In the following investigations concerning the influence the different minerals associated with the black tin have on the results of these assays, we shall see to which of the two methods the final preference is to be awarded.

V. Influence of Associated Minerals on the Assay.

The minerals that are liable to remain with the black tin after the ore has been washed, roasted, and treated with nitro-hydrochloric acid, are quartz, feldspar, mica, tourmaline, garnet, and columbite (tantalite). To test their influence, the minerals were ground fine enough to pass through a 60-mesh sieve, and any particles of iron coming from the implements used in grinding were removed by the magnet. The minerals were mixed with the black tin in the different proportions shown in the subjoined tables, and the effects studied which they have on the two best methods of assay, — the German method and the method of fusing with potassium cyanide. The charges were so regulated that the flux in the German assay was always equal to three times the quantity of the ore plus the mineral, and in the cyanide assay to six times the quantity. Four parts of cyanide were mixed with the ore; one part served as bottom for the crucible and one part as cover. In the German assay, the amount of borax glass was increased in proportion to the percentage of mineral added and that of salt decreased. The fusions were made in the usual way; the time for the cyanide assay had to be increased to not less than fifteen minutes before a tranquil fusion was obtained.

(1.) The quartz used was the ordinary compact, colorless variety.

The amount of soft slag found on top of the borax slag decreased in proportion to the percentage of quartz added, showing that part of it had entered into combination with the borax slag. In assay No. 111 it was ¼ inch thick, in No. 115 only ⅛ inch. Its structure was not so coarsely crystalline as usual. It grew finer, and the brittle slag became harder with increase of silica; the lustre remained the same; the color varied from gray to bluish-black. The borax slags increased in quantity as quartz was added, their vitreous character being somewhat impaired by the silica, and also their brittleness; the lustre remained uniform; the usual olive-green color changed somewhat and became brownish. The buttons separated well from their slags, had the usual appearance, and

were free from iron. The results obtained in Nos. 111 to 115 showed a gradual decrease in the amount of tin recovered. This is, however, less than has been generally supposed from the figures of Berthier, Nos. 116 to 120, which are usually quoted.[1] Berthier,[2] as the writer understands it, mixes his siliceous ore with four times the amount of black flux. With the German method of charging, the black tin with

TABLE XXII.

No. of Assay.	Charge.							Resulting Tin.	
	Ore.	Quartz.		Charcoal.	Bl. Flux Substitute.	Borax Glass.	Potass. Cyanide.		
	Grams.	Grams.	Per cent.	Grams.	Grams.	Grams.	Grams.	Grams.	Per cent.
111	5	1	16.66	1	18	1	....	3.280	65.60
112	5	2	28.57	1	21	1	....	3.100	62.00
113	5	3	37.50	1	24	....	....	3.035	60.70
114	5	4	44.44	1	27	....	....	2.945	58.90
115	5	5	50.00	1	30	....	....	2.565	51.30
116	10	2.5	20.00	....	Bl. Flux. 50	....	....	5.2	52.00
117	10	6.6	40.00	....	66.4	....	....	4.3	43.00
118	10	10.0	50.00	....	80	....	....	2.8	28.00
119	10	15.0	60.00	....	100	....	....	1.0	10.00
120	10	30.0	75.00	....	160	....	....	0.0	00.00
121	5	1	16.66	....	....	....	24	3.285	65.70
122	5	2	28.57	....	....	....	28	3.215	64.30
123	5	3	37.50	....	....	....	32	3.195	63.90

67.84 per cent of tin mixed with 50 per cent of silica gave 51.30 per cent of metallic tin; Berthier's black tin containing 70 per cent of tin, with the same addition of silica, but mixed with the flux, gave only 28 per cent of tin or 24.8 per cent less than is charged as in the German method. This shows the disadvantage of mixing ore with basic flux.

[1] Mitchell, *op. cit.*, p. 483; Ricketts, *op. cit.*, p. 87.
[2] Traité des Essais, 1847, Vol. II. p. 485.

In the cyanide assays, Nos. 121 to 123, the slags showed uniformly a very coarsely crystalline structure, and had a strong lustre. They were as opaque as usual, and became harder with the increase of silica. In No. 121 only the upper rim was parrot-green. This spreads in No. 122 so as to cover the entire surface; it is ¼ inch thick at the circumference and ⅛ inch in the centre; in No. 123, the thickness is uniformly ¼ inch. The appearance of the buttons is the same as if no silica had been added. The results show a decrease in the amount of tin recovered, but the loss is not so great as with the German assay.

(2.) The feldspar used was the compact albite that occurs so generally in the tin lodes; it has a very perfect cleavage, shows a pearly to a silky lustre, and is white with a tinge of gray.

TABLE XXIII.

No. of Assay.	Charge.							Resulting Tin.		Resulting Alloy.	
	Ore.	Albite.		Charcoal.	Bl. Flux Substitute.	Borax Glass.	Potass. Cyanide.				
	Grms.	Grms.	Per cent.	Grms.	Grams.	Grams.	Grams.	Grams.	Per cent.	Grams.	Per cent.
124	5	1	16.66	1	18	1	....	....	....	3.475	69.50
125	5	2	28.57	1	21	1	....	....	....	3.265	65.20
126	5	3	37.50	1	24	1.25	....	....	....	3.010	60.20
127	5	4	44.44	1	27	1.50	....	....	....	2.680	53.60
128	5	5	50.00	1	30	2	....	....	....	3.615	52.30
129	5	1	16.66	....	....	....	24	3.285	65.70	....	....
130	5	2	28.57	....	....	....	28	3.275	65.50	....	....
131	5	3	37.50	....	....	....	32	3.265	65.30	....	....

Salt and borax slags of the German method, Nos. 124 to 128, are similar to those obtained when quartz was added to the ore. The buttons, however, are surrounded by a slag that is more stony than the borax slag above, and almost black. It adheres strongly to the buttons, and is liable to retain particles of metal. All the buttons were dark and dull; they were brittle, hard to cut, and contained iron, its presence being caused by the infusible character of the albite; the reduction was prolonged and the ferric oxide of the cassiterite reduced before fusion took place, and alloyed with the metallic tin resulting from the more readily reducible stannic oxide. By examining the results, Nos. 124 to

128, it will be seen that, although all the buttons are ferruginous, the weight of the last four buttons is too low, showing that tin had also entered the slag, not enough iron being present to precipitate all the tin. The slagging is assisted by the intimate contact of the refractory feldspar and the cassiterite. Owing to the character of the charge, stannic oxide, or even metallic tin, is liable to be slagged by the alkali carbonate, and stannous oxide by the silica.

The influence on potassium cyanide is seen by Nos. 129 to 131 of the table. The upper white slag of pure cyanide with cyanate adheres to a lower porous slag of greenish-gray color, which encloses the buttons. These are white, bright, malleable, easily cut, and free from iron. The amount of tin lost, even if 37.50 per cent of albite is added to the ore,

TABLE XXIV.

No. of Assay.	Charge.							Resulting Tin.		Resulting Alloy.	
	Ore.	Muscovite.		Charcoal.	Bl. Flux Substitute.	Borax Glass.	Potass. Cyanide.				
	Grms.	Grms.	Per cent.	Grms.	Grams.	Grams.	Grams.	Grams.	Per cent.	Grams	Per cent.
132	5	1	16.66	1	18	1	....	....	....	3.190	63.80
133	5	2	28.57	1	21	1	....	....	....	3.250	65.00
134	5	3	37.50	1	24	1.5	....	....	....	3.200	64.00
135	5	4	44.44	1	27	1.5	....	....	....	3.605	72.10
136	5	5	50.00	1	30	2	....	....	....	....	....
137	5	1	16.66	....	....	....	24	3.260	65.20	....	....
138	5	2	28.57	....	....	....	28	3.235	64.70	....	....
139	5	3	37.50	....	....	....	32	3.165	63.30	....	....

is only a little over 2 per cent. It is strange that there should be so little difference between the three assays which contain such varying amounts of albite. The only explanation seems to be, that, at the low temperature at which the assay is carried out, the feldspar is not decomposed, but remains suspended in the lower slag, which receives its greenish tinge from the iron of the cassiterite and its gray from the feldspar mixed with it. If the lower slag is boiled in water, it falls to pieces, leaving a residue that looks very much like the original pulverized albite after it has been moistened, except that it has a slight tinge of green, resulting from the iron.

(3.) The mica used was a pure white muscovite.

The general behavior of the assays according to the German method, Nos. 132 to 136, is the same as those with feldspar, the exception being that with 50 per cent of mica only an incomplete fusion could be obtained. In the bottom of the crucible was a porous, fritted, dark mass, no tin being visible; it was covered by a semifused, scoriaceous, spongy mass of a brown color. The buttons had the same appearance and properties as those from the feldspar mixture. The results show a greater loss of tin and a stronger reduction of iron than above, the reason being that muscovite is more refractory in the assay than albite.

In the assays with potassium cyanide, Nos. 137 to 139, the behavior of mica is similar to that of feldspar, the mica appearing also here more refractory.

TABLE XXV.

No. of Assay.	Charge.							Resulting Tin.		Resulting Alloy.	
	Ore.	Tourmaline.		Charcoal.	Bl. Flux Substitute.	Borax Glass.	Potass. Cyanide.				
	Grms.	Grms.	Per cent.	Grms.	Grams.	Grams.	Grams	Grams.	Per cent.	Grams.	Per cent.
140	5	1	16.66	1	18	1	....	....	....	3.470	69.40
141	5	2	28.57	1	21	1	....	....	....	3.300	66.00
142	5	3	37.50	1	24	1.5	....	....	....	3.250	65.00
143	5	4	44.44	1	27	1.5	....	....	....	2.295	45.90
144	5	5	50.00	1	30	2	....	....	....	....	....
145	5	1	16.66	....	....	....	24	3.285	65.70	....	....
146	5	2	28.57	....	....	....	28	3.250	65.00	....	....
147	5	3	37.50	....	....	....	32	3.235	64.70	....	....

(4.) The tourmaline pulverized for the experiments was the large black crystal which occurs so often with the tin ore.

In the German method, Nos. 140 to 144, the upper crystalline slag is parrot-green, the lower slag is slightly vitreous, but quickly becomes stony with the increase of tourmaline. The buttons have all a gray surface; they are dull, brittle, hard to cut, and ferruginous. These properties increase with the addition of tourmaline. In assay No. 144 no complete fusion could be obtained, and no button or particles of alloy were visible in the porous, fritted, dark mass. The results show

a loss of tin increasing with the addition of tourmaline, and an increased amount of iron is taken up by the tin.

With potassium cyanide tourmaline has no effect that is essentially different from that of previous assays, as the slags show about the same appearance, and the buttons the same properties and nearly the same weight.

(5.) The garnet occurring in the tin deposits is the iron garnet (almandite). It has a brownish red to fine deep red color, and is very rich in iron. This appears in its high specific gravity, 4.149, and from the fact that, after being heated by itself in a crucible to a bright red heat, it shows magnetic properties. For the experiments transparent crystals of a fine deep red color were pulverized.

TABLE XXVI.

No. of Assay.	Charge.							Resulting Alloy.	
	Ore.	Garnet.		Charcoal.	Bl. Flux Substitute.	Borax Glass.	Potass. Cyanide		
	Grams.	Grams.	Per cent.	Grams.	Grams.	Grams.	Grams.	Grams.	Per cent.
148	5	1	16.66	1	18	....	....	3.685	73.70
149	5	2	28.57	1	21	....	....	3.720	74.40
150	5	3	37.50	1	24	....	....	3.695	73.90
151	5	4	44.44	1	27	....	....	3.930	78.60
152	5	5	50.00	1	30	....	....	4.125	82.50
153	5	1	16.66	....	....	....	24	3.365	67.30
154	5	2	28.57	....	....	....	28	3.435	68.70
155	5	3	37.50	....	....	....	32	3.490	69.89

From the table it will be seen that no borax was added to the charges of the German assay, Nos. 148 to 152, as the garnet with the addition of a small amouut of base (lime or alkali) forms a liquid slag. The salt slags were less crystalline than usual, but otherwise the same with the exception of the color, which varied from light to dark red. The borax slags lost their vitreous and brittle character by the addition of garnet, became stony and tough, and changed from subtranslucent to opaque. The color, a dull olive-green, became lighter, the last slag, No. 152, being peacock-green. The buttons were all a dull dark gray, hard to cut, brittle, and rich in iron. The weights

obtained in the first four assays, Nos. 148 to 151, are strikingly close. This would seem to show that about the same amount of iron had been present in all of them, forming an alloy of nearly the same composition, the rest of the iron having remained in the slag. The uniformly well melted charges and the beautiful manner in which the one-button separates from the slag above, show that, if tin is to be recovered from siliceous ore as an alloy, a basic silicate of iron is the best, because it gives up the necessary iron for the tin and also retains sufficient in combination to form with the alkali of the charge a readily fusible, brittle slag.

In the cyanide assays the compact, white upper slag decreases, and the porous, bluish-green lower slag increases as garnet is added to the charge. In No. 153 the proportions were half and half, in No. 155 one quarter white and three quarters gray. The buttons are all ferruginous and show the usual characteristics. The figures prove that, although iron has been taken up, tin has gone into the slag.

(6.) The columbite found with the tin ores has an iron-black color and a streak that varies from a very dark brown to a pure black. The pure mineral has the following composition[1]: —

	Per cent.
Ta_2O_5	18.20
Nb_2O_5	64.09
FeO	11.21
MnO	7.07
SnO_2	0.10
CaO	0.21
Total	100.88

The specific gravity varies between 5.89 and 6.12. The columbite used for the experiments contained a small proportion of albite.

In the German assays, Nos. 156 to 160, the salt slags were stony and dull; their color, at first a brownish-green, gradually lost the brown, and became finally parrot-green. The borax slags were vitreous, brittle, and had a resinous lustre, the color changing from a light to a dark olive-green. The buttons were all ferruginous; their color, at first a dark gray, became black. The results show that the amount of iron reduced increased with the percentage of columbite added to the charge. Also, that if tin ore containing columbite be treated on a large scale, the columbite will probably behave in a way similar to the wolframite of Cornwall ores, where the iron that alloys with the

[1] Trans. Am. Inst. Mining Engineers, Vol. XVII. p. 593.

tin is the principal cause of trouble, and not the tungsten of the mineral, which, according to Berthier,[1] may be present in considerable quantities.

In the experiments with potassium cyanide the upper slags showed the usual properties, the lower slags were very porous, slightly glassy, and had a dark blue-gray color. The porosity increased with the percentage of columbite added. The buttons were white, bright, malleable, easily cut, and contained very little iron; from their appearance they might have been supposed to be free from iron, until especially examined for this metal. If pure cassiterite and pure columbite are

TABLE XXVII.

No. of Assay.	Charge							Resulting Tin.		Resulting Alloy.	
	Ore.	Columbite.		Charcoal.	Bl. Flux Substitute.	Borax Glass.	Potass. Cyanide.				
	Grms.	Grms.	Per cent.	Grms.	Grams.	Grams.	Grams.	Grams.	Per cent.	Grams.	Per cent.
156	5	1	16.66	1	18	1	....	....	....	3.400	68.00
157	5	2	28.57	1	21	1	....	....	....	3.605	72.10
158	5	3	37.50	1	24	1.5	....	....	....	3.775	75.50
159	5	4	44.44	1	27	1.5	....	....	....	3.820	76.40
160	5	5	50.00	1	30	2	....	....	....	3.875	77.50
161	5	1	16.66	....	....	....	24	3.355	67.10	....	....
162	5	2	28.57	....	....	....	28	3.370	67.40	....	....
163	5	3	37.50	....	....	....	32	3.300	66.00	....	....

ground together and fused, thus intimately mixed, with potassium cyanide, the iron and manganese of the columbite combine with the tin, leaving a yellowish slag which contains the columbic and tantalic acids.[2]

By comparing the effect produced by the associated minerals in the two methods of assay, we see that the cyanide assay must be accorded the preference, although, if the amount of associated mineral be small, the German method will give entirely satisfactory results. In concentration works, where several assays have to be made daily, the question of cost must be considered, and the cheap German assay will probably

[1] Traité des Essais, 1847, Vol. II. p. 472.

[2] Drs. Carpenter and Headden, Trans. Am. Inst. Mining Engineers, Vol. XVII. p. 633.

be preferred to the more expensive one, especially if the assay is only made in order that the manager may know how pure his concentrates are. In questions connected with the buying and selling of ores or concentrates, the assay with potassium cyanide is better, because the associated minerals have comparatively little influence on it.

VI. Preparation of the Ore for the Assay.

It will be seen from the foregoing that the influence of the associated minerals on the result of the assay is very considerable. It becomes, therefore, necessary to have as pure a black tin as possible for the dry assay. When it is remembered that tin ores contain only from 0.75 to 3 per cent of cassiterite, their preparation for the assay is seen to be a very important part of the work.

The separation of the cassiterite from the gangue is universally accomplished by crushing and washing. The resulting concentrates are then purified by means which vary according to circumstances. From the great difference in specific gravity between the cassiterite (6.4 to 7.1) and the non-metallic associated minerals (not over 4.1), it would appear at first sight that the separation ought to be very easy, but this, for various reasons, is far from being the case. Special precautions are needed if the loss of black tin is to be reduced to a minimum. The metalliferous minerals have too great a specific gravity (e. g. arsenopyrite 6 to 6.4) to be removed by washing; roasting and acids have to be used. The columbite (tantalite), whose specific gravity varies from 5.89 to 6.12, remains finally with the black tin, as it is not attacked by ordinary acids at all.

The implements used in assay laboratories for pulverizing larger quantities of ore are a small hand-crusher and a pulverizing plate. The ore is usually crushed, sampled down, and then pulverized on the rubbing-plate or in a mortar. Experience has proved that the common method of grinding the whole sample to a uniform size is not suited to the tin ores of the Black Hills. The main reason for this is the preponderance of mica. Any one who has tried to pulverize mica, even if previously heated, will appreciate the difficulty. Happily, very little cassiterite occurs in the mica proper, which if ground for a short time only on the rubbing-plate will not show any black tin or other mineral. The pulverization must be carried on with the subsequent washing in view. The first rule in concentration is, that a good preliminary sizing should precede the separation by specific gravity. The second is, that the ore should be coarse to prevent loss in washing. The latter point

is strongly emphasized in the present case by the fact that the brittle cassiterite will be ground to a slime, while other minerals in the ore are only converted into sands. The writer's practice has been to screen while the ore is being pulverized, crushing and screening alternately with a graduated series of sieves, Nos. 20, 40, 60, and 80. The ore is crushed until all the minerals, with the exception of the mica, are fine enough to pass a 20-mesh sieve; the mica is then examined for any minerals adhering to it, and these are removed by grinding, and screened. The different screenings are now washed separately in the gold pan, and the tailings panned over. The concentrates obtained by the separate washings are put together, dried, and passed through a 60-mesh sieve, and the washing is repeated, the result being pure concentrates and tailings. These are panned over if any cassiterite appears.

The writer has done all his washing of tin ore in the gold-pan; but any other apparatus, be it the vanning shovel used in Cornwall, or the "Sichertrog" of the Saxon or Bohemian miner, will, with the necessary practice, give the same result. In panning, it is found very difficult to separate the cassiterite from the garnet without losing much tin, although there is a great difference in their specific gravities, viz. 6.4 to 7.1 and 4.1. It is almost impossible to effect a complete separation, as can be seen from the analysis of the average sample, where the insoluble residue, 9.39 per cent, is principally garnet. This is on account of the different way in which the minerals break. Dana[1] says that both cassiterite and garnet have an uneven, subconchoidal fracture, and that both are brittle; they also differ little in hardness (6 to 7 *vs.* 6.5 to 7.5). But the cassiterite crushes into splinters, and the garnet into a granular form. It is probably this difference that causes the difficulty in separating the two minerals; the preponderating particles of cassiterite form an irregular angular network, and prevent the heavy garnet from being carried off by the water with the other minerals that have a lower specific gravity. If garnets are present in any large quantities it will be necessary to adopt other means to remove them. Dana[2] says that all garnets, except ouvarovite, are decomposed with hydrochloric acid after ignition. That this may be complete, the mineral has to be ground very fine. It will be remembered that the black tin used for the experiments was roasted, chilled, and treated with nitro-hydrochloric acid, and the garnets were not entirely removed. If the garnet is decomposed, the removal of

[1] System of Mineralogy, 5th ed., New York, 1880, pp. 158 and 265.
[2] *Op. cit.*, p. 270.

the gelatinous silica, either by washing or by hydrofluoric acid, is simple.

The black tin obtained by washing still retains sulphides and arsenides. Berthier[1] recommended boiling with nitro-hydrochloric acid and burning off any sulphur that might have separated. Plattner[2] objects to this, as the sulphur would not all be eliminated. He reverses the process, roasting first and then treating with nitro-hydrochloric acid. Following Plattner, the black tin is roasted on a dish in a muffle, and, to assist the decomposition of the garnet, chilled in cold water after roasting; it is then boiled in a porcelain dish with nitro-hydrochloric acid, which will also remove particles of metallic iron coming from the implements used in grinding; when all the iron is dissolved, the solution is poured off, and any light particles washed away. If necessary, the concentrates can be transferred to a platinum dish and treated with hydrofluoric acid. This, however, will rarely be required.

The roasting seems to have a favorable effect on the cassiterite by making it more friable and more readily reducible. The unroasted cassiterite of the Black Hills is subtranslucent, brown to black, and very hard to pulverize the roasted mineral is opaque, of a more or less reddish brown, and easily pulverized. The change that takes place in the reducibility of the ore is shown by the following experiments.

TABLE XXVIII.

No. of Assay.	Name of Mine.	Per cent of Tin from Raw Ore.	Per cent of Tin from Roasted Ore.
168	Occidental	48.90	49.00
169	Glendale	65.30	65.60
170	First Find	65.30	65.80
171	Similias	75.00	75.00

The results show that in some cases (No. 171) no difference exists between raw and roasted concentrates, while in others it may amount to 0.5 per cent.

If the preliminary treatment of a tin ore is carried out in the manner described, there will be a minimum of loss in black tin.

[1] Traité des Essais, 1847, Vol. II. pp. 486, 487.

[2] Blowpipe Analysis, edited by Richter and translated by Cornwall, New York, 1885, p. 466.

VII. Conclusion.

From this examination of the different methods of assaying tin ores in the dry way, it becomes apparent that, if ordinary precautions are taken, the necessary treatment, including the preliminary preparation of the ore, is a comparatively simple matter; neither is the influence of the associated minerals so great, at least with the cyanide assay, as is generally supposed. Lastly, the results obtained show that the dry assay of purified black tin gives more accurate results than that of any other base metal, if the few correct methods, none of them requiring special skill, are followed and ordinary care is given to the work.

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THE CHANGES OF THE YEAR.

BY FRANCIS A. WALKER, PRESIDENT.

THE erection of the Engineering Building properly belongs to the history of 1889, although the delay in putting up the great engine actually brought that work by a few weeks within the present year. Naturally enough, there is as yet little to be added to the story of the material development of the Institute. After such an effort as was involved in the erection of the building of 1889, a school as poor as ours may well look forward to resting for a little while. What has since been done is mainly of the nature of changes in the old buildings consequent upon the departure of the civil and mechanical engineers to their new and grander abode.

In the Rogers Building, Professor Richards has annexed to his domain all parts of the basement formerly occupied by Professor Lanza. These acquisitions nearly double the space devoted to mining and metallurgy, allowing not only a considerable extension of the assaying department, but also the equipment of an ample and safe blowpipe-room and the establishment of a small but pleasant library and reading-room. If the teachers and students of mining and metallurgy are not happy now, it will be useless to try to please them. Professor Richards retains the two rooms on the third floor hitherto occupied by him, the one as a lecture-room, the other as a model-room and museum. Another change, not less welcome to others than to the miners, has been the removal of the 80 horse-power Porter-Allen engine, which for seven years has kept pounding away in the basement, shaking the whole building and greatly disturbing the occupants of the first floor, at times rendering the

Society of Arts room almost unfit for lectures. Professor Richards now has his own separate small engine, of 15 horse-power, while the power required for ventilation and other purposes in the Clarendon Street building is furnished by another engine, of the same power, situated in that building itself.

Another change, which will, it is hoped, bring both pleasure and advantage to the students of the Institute, has been wrought on the fourth floor of Rogers, by which the whole front of that story has been converted into a large, light reading-room for students in English, history, and political science. For this purpose, (1) the old and most unsafe blowpipe-room, (2) Professor Schwamb's dark and gloomy model-room, and (3) the mathematical recitation-room on the southeast corner of the building, were thrown into one; book-cases and reading-desks were brought up from below or newly constructed; the lighting and ventilating of the room were improved as far as possible; and the virtue of paint was tried to its utmost, with a very agreeable result. The graduates of the past few years, who in their time were obliged to use the small, ill ventilated "English" reading-room in the third story, cannot fail to rejoice at learning under how much pleasanter conditions their successors in the school now read the reviews, look up the professors' references, or write out their notes. Professor Dewey has his own office in immediate connection with the new reading-room; the other teachers in this department keep theif desks in rooms 31 and 33, on the floor below, using these at times for small classes.

The first year mechanical drawing classes, under Professor Faunce, so long cooped up in the "lantern" story, now occupy the large room (No. 43) on the Newbury Street side of the fourth floor. This room has desks for 199 students working at a time, while the lockers and drawers provided give accommodation for 102 more. Here, also, are found chairs and reading-tables for thirty to fifty students of the first year, in the intervals between recitations and lectures. The room left vacant by this better provision for mechanical drawing has been taken up by the classes in freehand drawing, and has been greatly "spruced up" and liberally provided with special apparatus and appliances.

On the main floor, the lecture-room (No. 15) so long known as the Society of Arts room has been invaded by a much needed extension of the Secretary's office, the remainder of the room being turned into a students' reading-room, with a small lobby for the librarian and his assistant. The rising floor and chairs of room 15 have been carried across the hall-way into room 11, which has been fitted up in all respects like the old Society of Arts room, with the added advantage

of greatly improved ventilation, as all who attend the meetings of that Society will be glad to learn.

I cannot leave "Rogers," even in this brief survey, without alluding to the wonderful change which two coats of paint and some carpenters' and glaziers' work have wrought in the aspect of the main hall and the grand stairways. It was only when the building was seen in its new dress that any one of us came to realize how greatly it had, for twenty years, been disfigured by stained and battered walls. It is much to be hoped that the students in their goings out and comings in will respect what has been done.

In the Clarendon Street building there has been but little change. Both because the growing department of industrial chemistry required more space, and because the color-work done in that department required more light than could possibly be obtained in the basement, room 31, at the southwest corner of the third floor, has been taken as a color-room, and fitted up with a large amount of special apparatus for dyeing and bleaching.

Owing to the crowded condition of the organic laboratory, room 33 has been fitted up especially for organic combustions. The students occupying this room enjoy an almost inestimable privilege in the presence of Professor James M. Crafts, formerly Professor of Organic Chemistry in the Institute, who, upon the invitation of the Executive Committee, is there carrying forward some of the researches that have made his name so honorably known to chemists of both hemispheres. On the second floor, the architectural students have taken possession of the large drawing-room left vacant by the departure of the civil engineers; a door has been cut through from the architectural library; and this department, with more than one hundred students, now enjoys a beautiful and commodious suite of rooms, in every way adapted to its always advancing work.

Such have been the chief material changes in the Institute during the official year just closed. The changes in the *personnel* of the instructing staff, although not numerous except through the enlargement of the number of assistants in the laboratories, have been important. Mr. George R. Carpenter, of Harvard University, has been appointed Associate Professor of English, and has entered upon his duties under the fairest auspices. Professor Carpenter is especially a student of Comparative Literature; but with us he undertakes the difficult and delicate duty of giving to the students of a school of industrial science that instruction and practice in the use of language and in the art of composition which shall be most for their benefit, professionally and socially.

The problem is a difficult one by reason of the limited amount of time and strength which, in an institution of high scientific and technical aims, can fairly be devoted to this object; but Mr. Carpenter takes it up with a zealous interest which furnishes almost an assurance of success. His plan is to enter into such relations with the instructors in charge of the "professional" work of the school as will enable him intelligently to assign to each student themes having reference to his own studies, securing thus the great advantage of having the pupil write, simply, with direct intention, and out of a full mind, on subjects with which he is familiar and in which he is interested, instead of writing vaguely, affectedly, and ambitiously about subjects of which he knows little, and for which perhaps he cares less. In the course of this work, lists will be made of students who display unusual aptitude and pleasure in composition; and these will be offered an opportunity for advanced study and practice in this line, so far as they may find time to undertake it. More important still, lists will be made of those students, beginning with the very first year, who from lack of natural aptitude for such studies, or from defects of training, are found to be at a special disadvantage in English composition; and these students will be followed up with an unremitting but friendly importunity, to induce and enable them to correct their faults and supply their deficiencies on this side.

Professor Carpenter is assisted by Mr. Fred P. Emery, who for the past year has done so much to keep up and carry forward the instruction in English at the Institute, and who has shown a continual gain in power both as a scholar and as a teacher; and also by Mr. R. W. Herrick, of Harvard, a gentleman of high promise in literary work. The English Department thus becomes strongly officered, so that, with the admirable work now carried on by Professor Levermore in history, and by Professor Dewey in economics and statistics, the Institute of Technology becomes a place where the best instruction, with the use of the most enlightened methods and with adequate arrangements and appliances, will be given in these studies, which, if less important to the engineer or the chemist than to the lawyer or the writer for the press, cannot fail to be of great professional value to the former.

After a year's absence, Professor Letang has returned to his duties in the department of Architecture, cordially welcomed by all his former students. Assistant Professor Porter has been appointed Associate Professor of Hydraulic Engineering. Dr. Tyler, who patriotically gave up last year to the service of the Institute as Secretary to the Faculty, has been appointed Assistant Professor of Mathematics

He has been succeeded as Secretary by Mr. J. B. Henck, Jr., of the class of 1876.

Instructors Sondericker and Homer have been appointed Assistant Professors respectively of Applied Mechanics and Architecture. Doctors Gill, Talbot, and Noyes, returning from two years' study in Germany, have resumed their work of instruction in chemistry. Mr. Stanwood has been promoted to be Instructor in Civil Engineering. Mr. Collier Cobb, of the Harvard class of 1889, has been appointed Instructor in Geology.

It is a source of deep regret that Professor Holman has been compelled by the state of his health to relinquish his post for the whole of the academic year. Every past and present student of the Institute will join in the fervent wish that this accomplished scholar may return to his duties in 1891, completely restored to health and soundness. Another valued member of the Faculty, Professor Schwamb, is at this time suffering the effects of overwork, and may be compelled for a time to give up teaching and seek relief in a milder climate.

To the eleven distinct courses of instruction hitherto opened to the students of the Institute, one more has been added during the year, namely, that in Geology, to be known as Course XII. This was one of the departments in which our school was strongest at the beginning, as, indeed, might have been expected in an institution founded by Professor William Barton Rogers; but the steady increase in the requirements of mining engineering and metallurgy has had the effect more and more to overshadow instruction in geology, until it has been thought best to reassert the importance of this study by constituting a separate course in which this shall be the leading feature. A goodly amount of chemistry affords preparation for the geological work of this course, and in the later years topography takes a prominent place. The enriching and diversifying of the longer established courses has gone forward during the year, as occasion has arisen.

In the Civil Engineering Department the extension of the work in hydraulics, for which the new building offers such exceptional facilities, has been emphasized by Mr. Porter's appointment as Associate Professor of Hydraulic Engineering. A new option in Highway Engineering is to be instituted this year, with means derived from the liberal gift of Albert A. Pope. The department of Mechanical Engineering will carry all its Senior options further than ever before; while the instruction in Naval Architecture, begun in a modest way two years ago, will be largely developed under the charge of Professor Peabody. Last year and year before last, a course on the Stability of Ves-

sels was given by Assistant Constructor Woodward, of the U. S. Navy. The present year we hope not only to have those lectures repeated, but to give the students taking this option a series of exercises in application of the principles taught, for which complete sets of the drawings and computations for the construction of vessels of the best modern types are now in preparation.

The course in Mining Engineering and Metallurgy has been completely recast with reference to the work of the current school year. The students of that course have their choice among four different lines of work. The first course prepares a student to take hold of work in line for promotion in any mining industry, — coal, iron, gold, silver, copper, or lead, — as a miner, a foreman, assistant manager, or manager of a mine. It fits him in geology, mining, milling, and in mechanical engineering adapted to mining. The second course prepares a student to take hold of work in line for promotion in a surveyor's office in a mining district, as assistant surveyor, as surveyor of mines, as consulting mining engineer, as mine superintendent. It fits him in geology, mining, milling, and in civil engineering adapted to mining, construction of railroads, electric railroads, canals, roads, buildings, bridges, etc. The third course fits a student to take position in line for promotion in iron, lead, copper, zinc, gold, or silver works, as a hand, as foreman, assistant superintendent, or superintendent, or he may enter the works by the chemical laboratory. It fits him in analytical chemistry, metallurgy, mathematics, and in mechanical engineering adapted to metallurgical machinery, blowing engines, rolling mills, etc. The fourth course fits a student to take position in line for promotion in iron, lead, copper, or zinc works, as assistant chemist, as chemist, as furnace manager, as superintendent. It fits him in analytical and theoretical chemistry, in metallurgy, and in electrometallurgy.

This year will see the first class of students graduate from the course in Chemical Engineering (X.), instituted in 1888. Eight young men of the fourth year class are candidates for the degree of the Institute in this course. It will not be until next year that we shall have our first class of students graduate in Sanitary Engineering (Course XI.).

It is too early at this time to state the exact number of students in the Institute; much more, to offer the statistics of their distribution among classes, courses, etc., such as appear in the Annual Report of the President. There is a small increase in attendance over last year. An interesting fact is the presence of an unusual number of students from foreign lands, especially from Great Britain.

November, 1890.

LACTIC ACID AND LACTATES IN TEXTILE COLORING.

By LEWIS M. NORTON, Associate Professor of Organic and Industrial Chemistry, and HERBERT C. TUTTLE.

SUGGESTIONS have frequently been made regarding the use of lactic acid in textile coloring,[1] but until very lately it has not been able to compete in price with tartaric and citric acids. Furthermore, the cheaper grades of acid have not been colorless nor free from impurities capable of working injury in coloring. It is only within a few months that an acid sufficiently pure[2] for use in coloring has been produced at a cost which will enable it to be obtained at a reasonable price. Its use has become practicable, and the experiments described below were designed to throw light upon its effect. The samples used in this investigation contained about 43.5 per cent of lactic acid and were nearly colorless. The impurities present were sulphate of lime and a faint trace of iron.

Use in Wool Dyeing.

Our first experiments were made in regard to the use of acid lactate of potash in the place of tartar, and for this purpose the lactic acid solution was half neutralized with carbonate of potash.

Samples of wool were mordanted with the following proportions: —

	Per cent.		Per cent.
Bichromate of Potash . . .	2.5	Bichromate of Potash . . .	2.5
Tartar	2.5	Acid Lactate of Potash . . .	2.5

The samples were then dyed with alizarine. The sample mordanted with acid lactate gave a decidedly heavier shade of the same tint, indicating that more chrome was fixed upon the wool with the acid lactate than with the tartar. Samples were next mordanted as below, and dyed with alizarine: —

	Per cent.		Per cent.
Bichromate of Potash . . .	2.5	Bichromate of Potash . . .	2.5
Oxalic Acid	1.0	Lactic Acid	1.0

The sample mordanted with lactic acid was much deeper in shade, as in the first case.

[1] Avery, Phar. Journ., Aug. 5, 1882. See also Journ Soc. Chem. Ind., 1885, p. 116.

[2] Waite, U. S. Patents 365,655 and 368,032, 1887.

Samples of wool were next mordanted with chrome and alum as below: —

	Per cent.		Per cent.
Bichromate of Potash	1	Bichromate of Potash	1
Alum	10	Alum	10
Oxalic Acid	1	Lactic Acid	1

These were dyed with alizarine. The lactic acid gave a much deeper shade. It was, however, bluer than that furnished by oxalic acid, and perhaps less brilliant.

The result obtained by substituting lactic acid for tartaric acid in mordanting with tin for alizarine, was very striking. The samples were mordanted as follows: —

	Per cent.		Per cent.
Tin Crystals	10	Tin Crystals	10
Tartar	10	Acid Lactate	10

The color obtained with tartar was the well known reddish orange, while the lactate shade was a very brilliant yellowish-red of great beauty, such as it has hitherto been very difficult to produce upon wool with alizarine. It would appear that much more tin has been fixed in the case of the lactate. The effect of the lactate upon alum alone was next tried: —

	Per cent.		Per cent.
Alum	10	Alum	10
Tartar	10	Lactate	10

The samples were dyed in alizarine, and it was found that the color obtained with the lactate was very much bluer than that obtained with the tartar, and somewhat heavier. It was evident that in all cases the heavier shades obtained with lactic acid were probably due to the greater fixation of the base. To prove this, three baths were prepared with equal quantities of alumina present in them, and equal weights of tartar, acid oxalate of potash, and acid lactate of potash, and an equal weight of wool under exactly the same condition mordanted in each. The results were as follows: —

	Tartar. Per cent.	Acid Oxalate. Per cent.	Acid Lactate. Per cent.
Alumina fixed	63.5	71.8	80.0
Alumina remaining in the Bath	36.5	28.2	20.0

A similar series of experiments were made, except that copperas was substituted for alum: —

	Tartar. Per cent.	Acid Oxalate. Per cent.	Acid Lactate. Per cent.
Iron fixed	40.4	47.5	60.2
Iron remaining in the Bath	59.6	52.5	39.8

The samples were dyed with alizarine, and the results agreed with the figures furnished by the analysis, except that the difference in favor of the lactate mordant was somewhat greater than could have been caused by the fixation of the greater quantity of base. It appears that the presence of lactic acid exercises a favorable effect upon the depth of shade produced by alizarine upon wool.

The following mordanted samples were next prepared: —

	Per cent.		Per cent.		Per cent.
Chrome Alum	30	Chrome Alum	30	Chrome Alum	30
Tartaric Acid	15	Oxalic Acid	15	Lactic Acid	15

The samples were then dyed with alizarine. The lactic acid gave much the heavier and brighter shade of buff, the oxalic acid the second best, and the tartaric acid the poorest.

The results obtained on wool with woods upon samples mordanted with lactates were much less favorable. We are unable to assign any reason for this, but numerous experiments usually furnished this result, even where the mordanted wool had been washed with the greatest care. Our first experiments were made with logwood: —

	Per cent.		Per cent.
Bichromate of Potash	2.5	Bichromate of Potash	2.5
Tartar	2.5	Acid Lactate	2.5

The samples were dyed with 20 per cent of logwood. The acid lactate did not furnish as bright or as heavy a blue as the tartar. An oxalic acid blue was tried as below: —

	Per cent.		Per cent.
Bichromate of Potash	2.5	Bichromate of Potash	2.5
Oxalic Acid	1.0	Lactic Acid	1.0

The lactic acid blue was heavier and better than the oxalic acid blue, and superior to that obtained with acid lactate. With bark liquor the result was unfavorable: —

	Per cent.		Per cent.
Sulphate of Alumina	4	Sulphate of Alumina	4
Oxalic Acid	2	Lactic Acid	2

The samples were then dyed with 10 per cent of bark liquor. The oxalic acid gave a clear greenish yellow, the lactic acid a dull reddish yellow. With tin crystals the result was similar: —

	Per cent.		Per cent.
Tin Crystals	10	Tin Crystals	10
Tartar	5	Acid Lactate	5

These were dyed with 10 per cent of bark liquor. The acid lactate gave results much inferior in tone to those given by the tartar. Fustic was next tried. The wool was mordanted, as below: —

	Per cent.		Per cent.
Tin Crystals	8	Tin Crystals	8
Tartar	5	Acid Lactate	5

The wool was then dyed with 10 per cent of extract of fustic. The tartar gives a more orange shade, while the lactate gives a clear yellow.

Comparisons were made with camwood, as below: —

	Per cent.		Per cent.
Tin Crystals	8	Tin Crystals	8
Tartar	30	Acid Lactate	30

The samples were then dyed with 30 per cent of camwood. The lactate gave the redder and lighter shade.

Experiments were then made with cudbear in the proportions given below: —

	Per cent.		Per cent.
Cudbear	40	Cudbear	40
Tartaric Acid	2	Lactic Acid	2

The shade obtained with lactic acid is decidedly bluer than that given by tartaric acid, and somewhat fuller.

An experiment was made with logwood upon an alumina mordant, and sulphate of alumina, with tartar, acid oxalate of potash, and acid lactate of potash, used.

Per cent.	Per cent.	Per cent.
Sulphate of Alumina 4	Sulphate of Alumina 4	Sulphate of Alumina 4
Tartar 4	Acid Oxalate 4	Acid Lactate 4

These samples were dyed with 30 per cent of logwood. It was observed that the color took much more rapidly upon the sample mordanted with acid lactate, but the final result was decidedly in favor of the acid oxalate. The shade with the oxalate was deeper and clearer, while that produced with the lactate was much superior to that produced with tartar, both in depth and shade.

Use in Cotton Dyeing.

The employment of the double lactate of antimony and potassium as a substitute for the soluble antimony salts at present in use in cotton

dyeing was, so far as we are aware, first proposed by C. N. Waite,[1] several years ago. The high price of the acid and the difficulty in preparing a satisfactory mordant have prevented its introduction. We prepared the double lactate of potassium and antimony by dissolving the oxide of antimony in a solution of acid lactate of potassium, and evaporating this solution to dryness. The oxide of antimony was precipitated from a solution of oxymuriate by the addition of a large excess of water, the oxide carefully washed, and then added in excess to a solution of acid lactate of potassium, the excess removed by filtration and the liquid evaporated.

In all our experiments the cotton was worked for one hour in a solution of 25 per cent of sumach and then treated with a solution containing 2 per cent of the double lactate, and a similar skein was prepared where the tannin was fixed with an equal quantity of tartar emetic. The two skeins were washed and dyed together in baths containing 4 per cent of color. Comparisons were also made in two cases with Haen's salt, the fluoride of antimony and sulphate of ammonia.

Basic Coal-tar Colors.

The following colors were tried: — Brilliant green, Bismarck brown, safranine, methyl violet R and 5 R, chrysoidine, methylene blue and magenta, while an experiment was also tried with congo corinth. These experiments showed that in general the fixing power of the double lactate was not so great for equal weights as that of tartar emetic, as the shades obtained with it were usually lighter. In the cases of methylene blue and safranine, however, a slight advantage in favor of the double lactate was found, while in the case of chrysoidine the result furnished by the double lactate is greatly inferior to those obtained with tartar emetic and Haen's salt. As far as clearness of shade is concerned, the double lactate is in no way inferior to tartar emetic; in the case of safranine the shade obtained was yellower than with tartar emetic. With methyl violet the results furnished with the double lactate and Haen's salt were substantially identical, while with chrysoidine Haen's salt gave far better results, as mentioned above.

Cotton Printing.

In cotton printing lactic acid and its compounds can only find employment as competitors with the more expensive organic acids, such as tartaric acid and citric acid. At the present time the price of lactic

[1] Journ. Soc. Chem. Ind., 1886, p. 483.

acid forbids its use in competition with acetic acid. It was possible, however, that lactic acid might in certain cases produce results much superior to those produced by acetic acid, and we have therefore used lactic acid in some cases where acetic acid is commonly employed. The number of processes in which lactic acid could be of value is naturally somewhat limited.

Cochineal Pink.

Although very little employed, experiments upon this color were first made, and the following receipt was used as a basis: —

Cochineal Liquor 6° Tw.	25.00 grams.
Oxalic Acid	0.25 "
Alum	1.25 "
Starch	32.00 "
White Tartar	1.25 "
Water	200.00 "

A swatch was printed with this receipt, and a second swatch with the same receipt except that lactic acid was substituted for oxalic acid, and acid lactate of potash for white tartar. The swatches were steamed, after drying, at five pounds for thirty minutes, and then soaped. The lactic acid print was far superior in depth of shade and brilliancy to that produced with oxalic acid and tartar. If this color was much employed, lactic acid could be used to great advantage in its production.

White Discharge upon Turkey Red.

In some of our experiments upon discharging alizarine red with lactic acid, commercial Turkey red was employed, and in part of the experiments a red which we dyed ourselves was used. We observed a marked difference in the ease with which these reds could be discharged; the commercial sample was discharged with relative ease, while the sample which we dyed was discharged with much greater difficulty. We propose to make a series of experiments upon the relative fastness of different samples of alizarine reds, and upon the merits of different methods of dyeing them in this regard. Two discharges for Turkey red were prepared according to the following receipts: —

Tartaric Acid Discharge.

Alum	5 grams.
White Tartar	5 "
Tartaric Acid	60 "
Starch	16 "
Water	100 "

Lactic Acid Discharge.

Alum	5 grams.
Acid Lactate of Potash	5 "
Lactic Acid	60 "
Starch	16 "
Water	100 "

The two pastes, after straining, were printed side by side upon the red, and after drying were immersed in a solution of bleaching powder. While the discharge obtained was not altogether successful, the results obtained with lactic acid are quite equal to those obtained with tartaric acid, and we see no reason why lactic acid should not be substituted for tartaric acid in this style of work. A second experiment was now made, in which lactic acid alone was used. The commercial acid was concentrated by evaporation to a gravity of 30° Tw., and then boiled to a paste with wheat starch, cooled, strained, and printed upon the red, dried, and treated with bleaching powder solution. The result obtained was very favorable. An attempt was made to discharge the mordant upon cloth prepared with oil and alumina for dyeing in alizarine by the same process as that described above, but the result, instead of furnishing a red and white pattern, gave a pattern in light and dark red, after dyeing.

Yellow Discharge upon Turkey Red.

The two following receipts were used, and printed side by side, upon Turkey red: —

Citric Acid Discharge.		Lactic Acid Discharge.	
Citric Acid Solution, 30° Tw.	80 c.c.	Lactic Acid, 30° Tw. . . .	80 c.c.
Nitrate of Lead	25 grams.	Nitrate of Lead	25 grams.
Starch	6 "	Starch	6 "

The swatches were dried and immersed in a solution of bleaching powder, then washed, chromed, and soaped. The yellow discharges thus produced were identical. It appears that lactic acid can be substituted for citric acid in work of this description. A dark brown discharge upon Turkey red was similarly produced by raising the lead salt fixed, with sulphuretted hydrogen water instead of chrome.

Turkey Red with Aluminum Lactate.

Aluminum lactate was prepared by the double decomposition of lactate of barium with sulphate of alumina, and had a gravity of 10° Tw. This solution failed to furnish a good red. A basic aluminum lactate was prepared by dissolving the hydrated oxide in lactic acid. This solution stood 12° Tw. Pieces of Turkey red were dyed, upon one of which basic aluminum lactate at 12° Tw. was used, and upon the

other red liquor at 12° Tw. The pieces were carried together through the whole process except the treatment with the alumina salts, dyed, steamed, and soaped together. Several experiments of this description were made. The red produced by the lactate was a little lighter and yellower, but the difference between the results produced by the lactate and acetate is very small. In connection with these experiments, an alizarine red was dyed, in which butyrate of aluminum was employed. The red was bluer and heavier than that produced by red liquor. The blue shade may have been due to a trace of iron in the aluminum butyrate. It offers no advantage over the acetate.

The experiments described in this article would indicate that lactic acid may be used to considerable advantage in dyeing alizarine upon wool, and in cotton printing in the place of tartaric and citric acids.

MASS. INSTITUTE OF TECHNOLOGY,
May, 1890.

ON THE INFLUENCE OF THE STRENGTH OF THE MAGNET IN A MAGNETO TELEPHONE RECEIVER.

BY CHARLES R. CROSS, THAYER PROFESSOR OF PHYSICS, AND HARRY E. HAYES, A. B., S. B.[1]

THE present investigation is a continuation of the research described in a paper by Messrs. Cross and Williams, previously published in the Technology Quarterly (Vol. II. p. 265). With a magneto telephone transmitter it was found that, as the strength of the magnet was increased, the induced current produced by a definite motion of the diaphragm increased quite rapidly to a maximum, and then gradually decreased. With diaphragms of different thickness it was found that, as the field necessary to saturate the diaphragm increased in strength, the maximum value of the induced current occurred with a greater strength of field, and also that the value of this maximum current increased with the thickness of the diaphragm, other things remaining the same. One series of measurements, however, whose results are shown in curve No. 23, Figure 2, of that article, seemed to be at variance with this conclusion, a discrepancy which was explained on the supposition that the supports holding the cam-rod by which the diaphragm was thrust forward yielded to a material extent with the thicker and stiffer diaphragm, which, by lessening the motion of this, would produce the observed result of a diminished current.

A series of measurements was first made in order to ascertain whether the above conclusion was correct. The apparatus employed was identical with that described in the previous article referred to, but the supports carrying the cam-rod were much increased in rigidity. The same diaphragms were employed as in the earlier experiments. The results were such as to show that the explanation of the discrepancy suggested was the true one, as will be seen on an examination of Tables I., II., and III. The diaphragms used were of iron, and of the same thickness as in the previous experiments, viz. Nos. 21, 22, and 23, B. W. G., corresponding to a thickness of 0.032 in., 0.027 in., and 0.024 in., respectively.

It was feared, however, that further difficulty might be met with from yielding of the supports of the cam-rod when heavy diaphragms were used, so that a modification of the apparatus was thought to be

[1] Read at a meeting of the American Academy of Arts and Sciences, November 12, 1890.

desirable. The cam-rod was therefore provided with a brass diaphragm-holder, so that the diaphragm, being carried by this, was moved bodily forward by the longitudinal motion of the cam-rod. A series of observations made with the same diaphragms used in the experiment last described gave the results shown in Tables IV., V., and VI., which are in substantial accord with those reached with the earlier form of the apparatus as given in Tables I., II., and III. From these tables the curves shown in Figure 1 were constructed. The induced currents are represented by abscissas, the strength of the field by ordinates.

TABLE I.

DIAPHRAGM, SHEET IRON, No. 21.

Strength of Field.	Induced Current.	Strength of Field.	Induced Current.
.000	0.0	.384	105.7
.070	19.0	.441	102.0
.119	38.0	.492	101.3
.158	49.5	.584	101.0
.202	64.0	.637	98.0
.246	77.7	.740	92.3
.292	93.3	.900	88.5
.344	99.0		

TABLE II.

DIAPHRAGM, SHEET IRON, No. 22.

Strength of Field.	Induced Current.	Strength of Field.	Induced Current.
.000	0.0	.380	97.5
.070	19.5	.425	96.8
.105	35.5	.456	96.7
.141	39.7	.514	94.0
.176	47.0	.577	88.0
.222	63.3	.700	84.3
.258	73.7	.781	76.5
.306	86.0	.916	71.5
.344	94.0		

TABLE III.

DIAPHRAGM, SHEET IRON, No. 23.

Strength of Field.	Induced Current.	Strength of Field.	Induced Current.
.000	0.0	.325	86.3
.070	15.5	.364	90.0
.123	30.5	.492	82.5
.155	42.3	.558	76.0
.194	53.8	.637	66.5
.236	65.0	.700	61.5
.268	75.7	.900	50.8

TABLE IV.

DIAPHRAGM, SHEET IRON, NO. 21.

Strength of Field.	Induced Current.	Strength of Field.	Induced Current.
.000	1.7	.620	124.3
.123	28.3	.649	118.3
.194	44.0	.700	111.0
.262	55.8	.787	102.9
.306	63.9	.862	96.5
.384	82.3	.976	88.5
.466	98.5	1.213	68.5
.554	118.5		

TABLE V.

DIAPHRAGM, SHEET IRON, NO. 22.

Strength of Field.	Induced Current.	Strength of Field.	Induced Current.
.000	0.5	.514	86.7
.125	22.5	.596	91.4
.191	36.3	.654	89.0
.268	45.3	.716	81.5
.302	52.8	.807	77.0
.325	56.9	.983	69.3
.420	72.3	1.065	56.3
.466	84.3	1.235	52.3

TABLE VI.

DIAPHRAGM, SHEET IRON, NO. 23.

Strength of Field.	Induced Current.	Strength of Field.	Induced Current.
.000	0.0	.488	90.5
.126	20.8	.577	87.5
.134	34.9	.654	80.3
.266	43.8	.716	72.0
.302	52.4	.784	57.4
.337	62.5	.888	43.2
.412	83.0	1.036	30.5
.449	89.3	1.235	24.5

The numbers 21, 22, 23, affixed to the curves of Figure 1, indicate the thickness of the corresponding diaphragms by their gage number (B. W. G.). The curves show that with the diaphragms Nos. 21, 22, 23, the maximum induced currents were 124, 93, 91, respectively, corresponding to relative strengths of field 61, 56, 49. The results reached in the earlier article are fully confirmed by these figures, which prove that, within the limits of the experiments, the maximum current is greater, and is reached with a stronger field, as the diaphragm is thicker.

An examination of the curves of Figure 1 shows that the point of maximum current is less distinctly marked with the thinner diaphragms than with the thickest. We are inclined to believe this to be an accidental peculiarity, due to a slight yielding of the diaphragm, which was fastened at its edges and which when thin would tend to bulge towards the magnet, thus causing its whole motion to be performed in a stronger field, and so giving rise to a stronger induced current than would otherwise be the case. An extremely slight displacement of this kind will produce a noticeable difference in the resulting induced current. This effect may also have acted to diminish somewhat

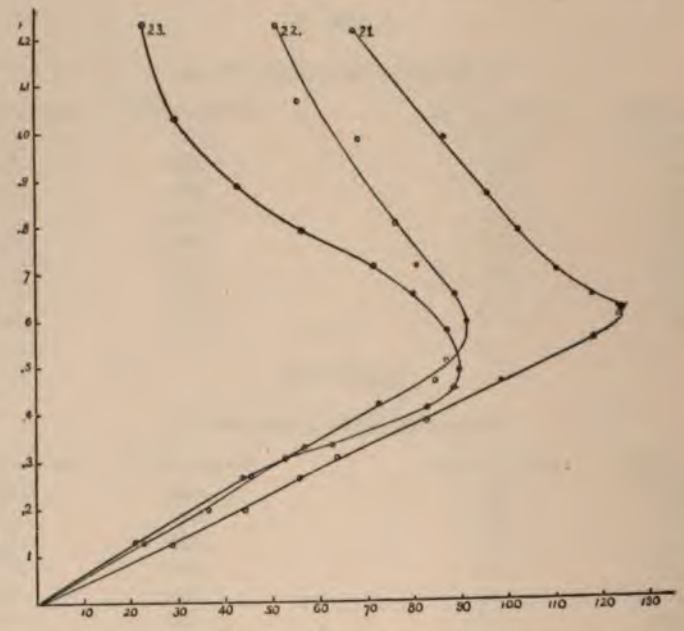

FIG. 1.

the difference between the maximum current obtained with the two thinnest diaphragms, which difference is much less than the difference between the maximum current with the two thickest ones.

Care was taken in all the experiments described in this paper that the magnet of the apparatus should be far below saturation, even with the strongest magnetizing currents which were used.

Our experiments were next directed to a study of the conditions operative in the magneto receiver, and particularly to ascertain the effect of a varying degree of magnetization in the magnet of the receiver upon the change in its strength when subjected to the action

of a slight current of the same order of magnitude as that used in telephony.

The apparatus employed consisted of a core of soft Norway iron, six inches long and a quarter of an inch in diameter, and provided with a suitable magnetizing coil. A diaphragm was placed opposite one end of the core, as in the usual magneto receiver. This end of the core also carried two coils of fine wire, like the ordinary telephone magnet coil, one having a resistance of about 130 ohms, the other of about 190 ohms. The former of these coils was connected with a storage cell, a fixed resistance, and a key. By means of this apparatus a current of suitable magnitude could be caused to traverse the coil, which evidently corresponds to the coil of the ordinary receiver, and which we have called the "line coil." The effect of this current will be slightly to increase or decrease the magnetization of the bar magnet, as in the receiver itself. The second fine-wire coil is a "measuring coil," and is connected in circuit with a ballistic galvanometer, from whose deflection when a current was sent through the first fine-wire coil the effect of this current on the magnet of the receiver could be determined. A magnetometer arranged in the usual manner showed the relative strength of the field of the receiver magnet under differing conditions of magnetization.

The observations consisted in adjusting the magnitude of the current through the magnetizing coil so as to give a certain deflection of the magnetometer needle. The magnetizing circuit included a suitable resistance coil, by the use of which any desired current could readily be secured. The key was then pressed, so that a certain current flowed through the line coil. The proper value was given to this current by means of a rheostat. The induction occurring in the measuring coil gave rise to a transient current through this circuit, the value of which was known from the deflection of the needle of the ballistic galvanometer included in it. Of course the key had to be kept down until the observation was completed, and it was also necessary, on account of the self-induction of the measuring circuit, always to read either on opening or on closing the key. In our experiments the latter alternative was chosen.

The results were plotted as previously by using the relative strength of the field as expressed by tangents of the magnetometer deflections as ordinates, and the induced currents in terms of an arbitrary unit as abscissas. The deflection of the ballistic galvanometer used was proportional to the strength of the transient current passing through its coils.

A series of observations was first made with the diaphragm removed, in order to study the deportment of the core alone under varying degrees of magnetization. The results are given in Table VII., and are shown graphically in Curve I, Figure 2. It appears from these, that with increasing magnetization of the core the induced current remains nearly constant for a time, and then diminishes rapidly and at an increasing rate, which afterwards becomes nearly constant, a result which would be anticipated from the corresponding diminution in the magnetic change which a small current in the line coil would produce.

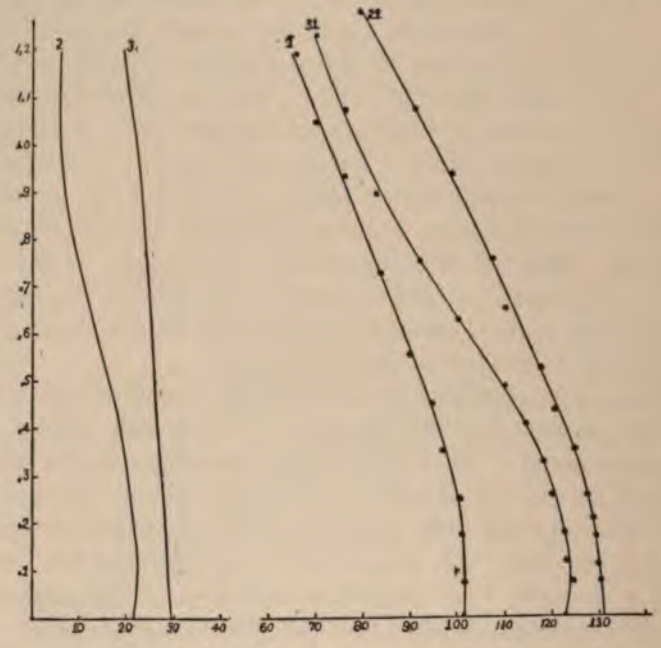

FIG. 2.

The current which traversed the line coil was six milliamperes, a value which was used in most of the measurements, it having been found by trial that the general character of the results was the same as when a smaller line current was used, while the greater magnitude of the deflection of the current induced in the secondary measuring circuit allowed more satisfactory readings to be made than if the line current were smaller.

A corresponding series of measurements with a diaphragm of ferrotype iron, No. 31, B. W. G., 0.010 in. in thickness, gave the results found in Table VIII. and shown in Curve 31, Figure 2. Curve 2,

Figure 2, is constructed with abscissas equal to the difference of the corresponding abscissas of Curves 31 and 1, and shows for various strengths of field the increase of the induced current due to the presence of the diaphragm, an interesting illustration of the action to which Du Moncel ascribed the chief efficacy of the diaphragm.

In order to study the effect of the thickness of the diaphragm, further measurements were made, using sheet iron diaphragms Nos. 21, 22, 23, 26, B. W. G., whose thickness was 0.032 in., 0.027 in., 0.024 in., and 0.018 in., respectively. The results of these measurements will

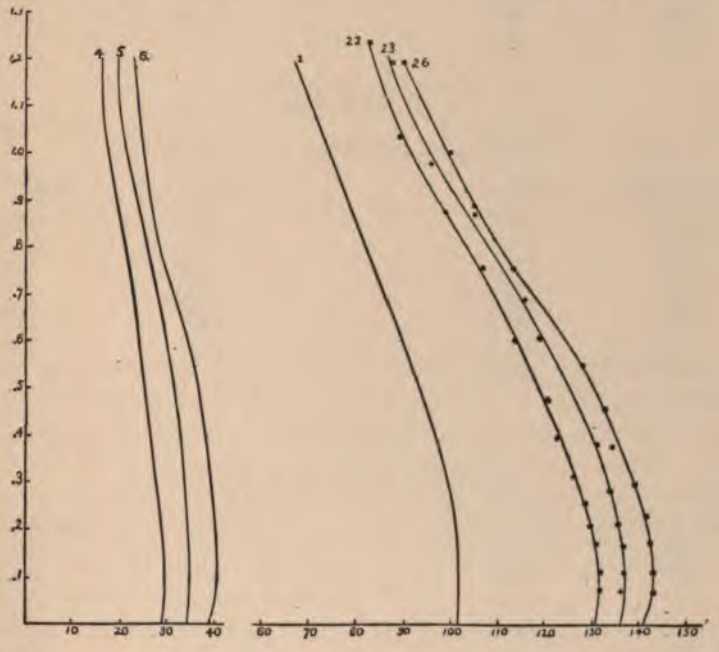

FIG. 3.

be found in Tables IX. to XII. inclusive, and from them the Curves 21, Figure 2, and 22, 23, 26, Figure 3, were constructed. The numbers attached to the curves indicate the gage of the diaphragm. The curves numbered 3, 4, 5, 6, respectively, are constructed to show the effect due to the addition of the diaphragm of thickness indicated by the gage number, as already explained in the case of Curve 2, Figure 2.

Several series of measurements were made with currents of widely different strengths, and with the thinnest and the thickest of the diaphragms used in these experiments, in order to ascertain whether the induced current in the measuring coil was proportional to the inducing

line current, and since this was found to be sensibly the case, the strength of six milliamperes was adopted, as already stated.

TABLE VII.

NO DIAPHRAGM.

Strength of Field.	Induced Current.	Strength of Field.	Induced Current.
.000	101.5	.554	90.7
.070	101.8	.727	84.3
.167	101.2	.933	77.6
.244	101.0	1.043	71.8
.348	97.8	1.192	67.7
.445	95.2		

TABLE VIII.

DIAPHRAGM, FERROTYPE IRON, No. 31.

Strength of Field.	Induced Current.	Strength of Field.	Induced Current.
.000	123.0	.488	110.9
.072	124.4	.625	101.7
.112	123.4	.754	93.2
.176	123.0	.897	84.0
.259	120.5	1.072	77.9
.327	118.7	1.235	72.0
.402	115.4		

TABLE IX.

DIAPHRAGM, SHEET IRON, No. 21.

Strength of Field.	Induced Current.	Strength of Field.	Induced Current.
.000	131.0	.437	121.7
.073	130.2	.521	118.4
.107	130.0	.649	111.3
.167	129.7	.754	108.5
.207	129.2	.933	100.2
.257	128.0	1.076	92.2
.356	125.4	1.280	81.4

TABLE X.

DIAPHRAGM, SHEET IRON, No. 22.

Strength of Field.	Induced Current.	Strength of Field.	Induced Current.
.000	130.8	.394	122.2
.073	131.4	.475	120.2
.112	131.6	.601	113.9
.169	130.2	.754	106.4
.204	129.2	.875	98.4
.255	128.2	1.039	88.7
.313	126.0	1.235	82 7

TABLE XI.

DIAPHRAGM, SHEET IRON, No. 23.

Strength of Field.	Induced Current.	Strength of Field.	Induced Current.
.000	136.0	.488	128.4
.068	136.0	.603	118.4
.107	136.7	.690	115.5
.167	136.4	.869	104.9
.211	135.7	.979	95.4
.281	133.9	1.196	87.2
.380	130.9		

TABLE XII.

DIAPHRAGM, SHEET IRON, No. 26.

Strength of Field.	Induced Current.	Strength of Field.	Induced Current.
.000	140.9	.454	132.2
.068	142.7	.543	128.0
.112	142.4	.625	124.7
.175	142.0	.754	113.0
.229	141.4	.885	104.4
.298	138.7	1.004	99.2
.376	134.0	1.196	89.2

Considerable trouble was experienced in obtaining satisfactory consecutive readings of the deflections, on account of the close proximity of the laboratory to the streets, rendered especially disturbing by the presence of the electric railway. The difficulty was avoided as far as possible by working at night. In order to indicate the character of our results so far as the deflections of the ballistic galvanometer are concerned, the values are given in Table XIII. from which the deflections in Table XII. are derived.

It will be seen from inspection of any one of the various curves last referred to, that as the strength of the field increases, the induced current increases, and then gradually diminishes. This would of course be anticipated, as the increased magnetization of both the core and the diaphragm increases the strength of field in which the latter moves, and at the same time diminishes the susceptibility of both masses of metal to magnetization. The effect of the former predominates slightly at first, but is overpowered by the latter cause when the magnetization is increased. The Curves 2, 3, 4, 5, 6, show that the addition of the diaphragm causes a greater increase of the induced current at low than at high magnetizations of the core, as would naturally be anticipated.

Curves 21 and 22 are so nearly alike that they have been placed in separate figures to avoid confusion. It would seem from this coinci-

dence that above No. 22 an increased thickness of diaphragm causes little effect.

TABLE XIII.

Deflections.			Mean.	Half-deflections.
A	B	C		
281	283	281	281.7	140.9
285	286	285	285.3	142.7
285	284	285	284.7	142.4
284	284	284	284.0	142.0
283	283	282	282.7	141.4
277	278	277	277.3	138.7
269	268	267	268.0	134.0
263	265	265	264.3	132.2
256	257	255	256.0	128.0
249	250	249	249.3	124.7
226	225	227	226.0	113.0
210	208	208	208.7	104.4
199	199	197	198.3	99.2
178	178	179	178.3	89.2

A comparison of the curves will show that, with the exception of Curve 31, for which the diaphragm was of ferrotype iron, the value of the maximum induced current is greater according as the diaphragm is thinner; and also that the strength of field corresponding to this maximum current is likewise greater the thinner the diaphragm. The following approximate values were found from constructions of the curves on a large scale.

For No. 26, Maximum Current = 142, Field = .18
" " 23, " " 137 " .13
" " 22, " " 132 " .10
" " 21, " " 131 " .06

It also appears that with the thinner diaphragms the decrease of the induced current with increase of field, after the maximum value of the current is reached, is more rapid than with the thicker ones. Moreover,

the deviation of the latter portion of the curve from a straight line is greater in the case of the thinner diaphragms.

The greater value of the maximum induced current with the thin than with the thick diaphragm would naturally be ascribed to the less stiffness of the former, and its consequent greater motion when a current passes through the line, although its smaller mass and greater ease of saturation must in a measure counterbalance this effect. The quicker approach of the thin diaphragm towards saturation will explain the more rapid falling off of the current with it, and also the less uniform rate of this change as the field increases. Curves 3, 4, 5, 6, showing the effect of the diaphragm, still further illustrate these facts.

It should be observed that the results which we have described do not necessarily show the varying acoustic effect of the receiver, since the amplitude of the motion of the diaphragm rather than the simple amount of the change of strength of the magnet determines the magnitude of the resulting air-waves, — at least according to the view commonly held. We hope to return to the study of this part of the problem, which is rendered somewhat difficult on account of the very small motion of the diaphragm.

ROGERS LABORATORY OF PHYSICS,
October, 1890.

THEORIES AS TO THE ORIGINAL HOME OF THE INDO-EUROPEAN RACE.

BY G. THEODORE DIPPOLD, INSTRUCTOR IN MODERN LANGUAGES.

IT was a common belief during the Middle Ages and the two centuries following that period, that the language of the Old Testament was the original language of the whole human race. Although a few scholars now and then faintly protested against this view, the honor of first having successfully attacked this erroneous theory belongs to the German philosopher Leibnitz (1646 to 1716). Towards the end of the eighteenth century the opinion of Leibnitz gained ground, and the most famous linguist of that time, J. C. Adelung, wrote, in 1781, that the Hebrew language was perhaps the oldest of which we possessed any considerable remains, but that it was not on that account the original language of the human race. Adelung, in common with most of his contemporaries, was convinced that the highlands of Central Asia formed the cradle of mankind. When he became acquainted with Sanscrit, he instinctively felt the great importance that language had in its relation to other languages; yet he promiscuously compared Hebrew, Turkish, and Syrian words with Sanscrit, German, Latin, and Greek. This desultory method of comparison was soon forgotten, after the first scientific work on the subject, Bopp's "Comparative Grammar," had appeared, in 1833. It became then gradually evident that the Sanscrit, Old Persian (Zend), Greek, Latin, Slavonic, Keltic, and Teutonic languages formed a family by themselves, distinct from all others, which has since been called the great family of Indo-European languages. For a short time the assumption prevailed that Sanscrit or Persian was the original language from which the others were descended. To the followers of either opinion it seemed comparatively easy to point out the earliest abode of the Indo-European race. To the supporters of Sanscrit, it was India; to those of Persian, the highlands of Media and Armenia. But it was soon shown, beyond doubt, that not any one of the languages mentioned above could claim to be the original Indo-European language, from which the others were derived, but that all of them stood to one another in the relation of sisters. Henceforth the solution of the problem as to what country was the earliest home of the Indo-European race became more difficult.

While the main work of the comparative philologists consisted in discovering, arranging, classifying, and explaining certain linguistic facts, their investigations necessarily led them to the question of the prehistoric unity of the Indo-European race. Yet, apart from its linguistic bearings, this question by its intrinsic historical importance has become very interesting, and deserves the attention, not merely of the specialist, but of all educated men.

The affinity of the Indo-European languages can only be explained by the assumption that the variety of forms [1] used to express one and the same idea or object must have arisen from a common source; or, in other words, at some prehistoric period there must have existed one original Indo-European language. If this theory be correct, there must have lived a people who spoke that language.

As has been stated above, the Sanscrit or the Persian was once considered to represent the earliest form of Indo-European speech. When this theory could no longer be upheld, it was nevertheless maintained that these two languages, since they apparently had preserved the purest and oldest forms, must have been spoken in closest vicinity to the earliest home of the Indo-European race. The greater the darkness enshrouding the first attempts at elucidating the treasures of Sanscrit and Persian (Zend), the brighter shone the halo of sanctity, originality, and antiquity which was supposed to encircle them. While the scholars who held this view differed as to the exact locality of the ancient abode of the Indo-European race, most of them assumed it to have been somewhere in Central Asia.

The theory of the Asiatic origin of the Indo-European race was entertained by the foremost scholars of the time, and was wellnigh undisputed until the beginning of the second half of this century, when all at once a doubt was cast upon it by R. G. Latham's "Elements of Comparative Philology" (London, 1862). In this work the author attempted to show, with more or less originality, that Europe was the primitive home of the Indo-European family. Prof. W. D. Whitney, in his "Language and Study of Language," in 1867, and in his later works, while strongly expressing his doubts as to the Asiatic origin of the Indo-European race, did not come out boldly in favor of the European theory, as Th. Benfey did, but confined himself to the state-

[1] Cf. Sanscrit *pitár;* Old Persian *pitar;* Greek πατήρ; Latin *pater;* Gothic *fadar;* English *father.* Sanscrit *mâtár;* Old Persian *mâtar;* Greek μήτηρ; Latin *mater;* Old Irish *máthir;* Old High German *muotar;* English *mother.* Sanscrit *duhitár;* Old Persian *dughdhar;* Greek θυγάτηρ; Gothic *daúhtar;* English *daughter.* Sanscrit *bhrâ'tar;* Old Persian *brâtar;* Greek φρήτηρ; Latin *frater;* Old Irish *bráthir;* Gothic *bróthar;* Slavonic *bratru;* English *brother.*

ment that neither language nor history gave us any clue to guide us in determining the earliest abode of the Indo-Europeans. The hypothesis of the Asiatic origin has not by any means been abandoned; on the contrary, some of the most distinguished scholars still adhere to it, but it can no longer be held up as incontestable. The discussion between the followers of the two antagonistic views has been carried on for the last twenty years, and although the strife has not been lacking in vigor and aggressiveness, it has never become a bitter and personal controversy, such as was witnessed, for instance, between Max Müller and W. D. Whitney a number of years ago, or in the early stages of the dispute in regard to the authorship of the *Nibelungen Lied*.

The old phrase "ex oriente lux," and the fanciful idea that the course of the sun and that of the progress of culture and humanity must be identical have no doubt exerted at times a fascinating influence on many minds. Nor have the attractions of comparative mythology been without bearing on the subject. The justly renowned scholar, Hans von Wolzogen, attempts to prove from certain mythological facts the descent of the Indo-European race from an Asiatic cradle. He starts from the well known Sanscrit myth of the battle of the god Indra with the fire-emitting dragons that have carried away the milk-dispensing cows. In this connection it would perhaps be interesting, but at the same time too far from our purpose, to enter upon the famous and sometimes ridiculed sun myth, and consider all the deductions drawn from it by Max Müller and his followers. It will suffice here to say that Indra represented the god of thunder, who freed the earth from the scorching summer heat (the dragons) which held the rain-giving clouds (the cows) imprisoned. In a Southern country this seems to be the natural way of explaining the myth; and therefore, according to Wolzogen, it must have been the earliest form of it, while the image as represented in Northern Europe appears to him distorted, and consequently must belong to a later age. In the Scandinavian myths and sagas, which to a great extent reflect the earliest era of Teutonic religion, the fire-emitting dragon (Fafnir) typifies the cold winter, which is conquered by the sun-god (Balder, Siegfried). Thus Wolzogen argues that the nation which preserved the myth in its primitive and natural shape must have proceeded from a region where image and reality coincided. This reasoning leads him to consider Northwestern India as the home of the Indo-European race.

On the other hand, the scholars who believe in the European theory protest, and with good reason, against the assumption that the Sanscrit and Old Persian languages contained the purest and earliest forms; it cannot be proved, and consequently all conclusions drawn from it are

doubtful or fallacious. There are several facts which demonstrate that the European languages, at least in one or two very important points, retained as great an originality as their Asiatic sisters.[1] The earliest runic inscriptions in Scandinavia show such a linguistic character as to entitle them wellnigh to a rank equal to that of the Sanscrit.[2] As to the occupation of East India by the Sanscrit-speaking tribes, we merely know that they came from the northwest. The Ganges, which at a later period, when the primitive purity and simplicity of their ancient religion had disappeared, became such a sacred river, is named but once in the Rigveda. On the banks of the Sindhu (Indus) were the chief settlements at the Rigveda epoch. Afterwards they gradually advanced farther south and east. It is also certain that at a remote period the ancestors of the Hindoos and Persians occupied the same regions, that is, the country to the north of the Himalaya.

But from these facts it does not follow that the *European* members of the Indo-European race must have come from the same territories; or, in other words, the Asiatic origin of the *whole* family cannot be proved from the mere fact that two Asiatic members of it at some distant epoch lived together somewhere to the north of the Himalayan Mountains. More than that, from the above facts we cannot even draw the final conclusion that the *earliest* abode of the forefathers of the Hindoos and Persians was in those regions, or in Asia at all. They may originally have come from Europe.

As to the European members of this great family of nations, it is beyond doubt that their earliest settlements, so far as known to us, were in Northern Europe; and in this connection it may be stated that the primitive abode of the whole Indo-European race must have been in the North. This is shown by the occurrence in the earliest languages of words for *snow* and *ice*, and the original division of the year into two seasons only, summer and winter. But the most important point in favor of the theory that Northern or Northeastern Europe was the first home of the Indo-European race, is the fact that the earliest migrations of the Indo-European tribes were on the whole in a direction towards the south, and often towards the east. The time has not yet come to decide this important question beyond all possibility of doubt; but at all events the long cherished Asiatic theory is not now accepted as infallible or as the only plausible one, and, to say the least, has lost much of its former prestige.

[1] The simple vowels *a*, *i*, *u* (pronounced as in Italian) of the Indo-Persian group, which contains no vowels corresponding to European *e* and *o*, were formerly considered to represent the earliest stage of Indo-European vocalism, while it is now apparent that the European vowel system *a*, *e*, *i*, *o*, *u* comes much nearer the primitive condition of the ancient language.

[2] Cf. Old Norse *ulfr*, Gothic *vulf-s*, and Sanscrit *vṛ'ka-s*.

PHOTOGRAPHY OF THE SOLAR PROMINENCES.

BY GEORGE E. HALE, S.B.

THE most remarkable developments of astronomy in recent years have been effected by photographic means. Thousands of star images secured upon a single plate; faint and far-reaching nebulæ clearly and permanently recorded; star spectra showing hundreds of lines; sun-spots in the greatest perfection of detail; — these are a few evidences of the great value of the sensitive plate to the astronomer. And the great success of the many investigators in this new field of research has so multiplied the applications of photography that there are few instances in which it has not already surpassed the most searching eye.

As photography has thus been employed in almost every department of astronomical research, it is rather to be wondered at that the solar prominences have as yet escaped its attacks. A simple and reliable method of photographing the prominences would not only do away with the tedious and inaccurate task of drawing, but might possibly discover new phenomena of value in clearing up the perplexing questions of the solar theory. A series of photographs of the same prominence, taken at exactly equal intervals of time, would be of much greater value than a similar series of drawings. And if the method could be made to serve in a daily registry of solar phenomena, its value would be beyond question. It might here be contended that the photograph would integrate all changes taking place during the exposure; but if this time could be made very short, the difficulty would disappear.

There are yet other opportunities for photography. In eye observations we are limited to the visible portion of the spectrum, while photography would lay open the infra-red and ultra-violet, where the prominences are known to have many lines. It would permanently register all the phenomena of the distortion and displacement of lines in the spectrum, and render their measurement a leisurely and exact process with the dividing engine; and it might ultimately lead to a more certain knowledge than we now possess of the true form of a prominence. In an observation or photograph through, for instance, the C line, what is shown is that portion of the prominence which is composed of incandescent hydrogen. A photograph taken through an

iron line, if one were visible at the time, would show the region occupied by iron vapor. In the same way, the parts played by calcium, sodium, magnesium, and other elements, might be shown; and a composite photograph from such a series of negatives would for the first time (outside of an eclipse) show the true form of the prominence, and the arrangement of its metallic strata. It is now known from comparisons of observations made just before and after eclipses with those during totality, that the C line gives the true form very nearly, if not exactly; but there is still room for much work in this direction, and in many others. For instance, Is the form seen through D_3 exactly the same as that seen through C? If not, are they more alike at some times than at others? Answers to questions such as these might render less mysterious the true nature of "helium," and define its relation to hydrogen. Tacchini's eye observations of prominence forms through several of the brighter lines show how advantageously photography might be employed.

Several attempts have been made at various times to photograph the prominences, but I will omit a discussion of the methods in this paper, merely remarking that no results of value have been obtained. Professor Young succeeded in obtaining the outlines of two prominences through an open slit; but the conditions under which they were made were not favorable, and no details were secured. Lohse's apparatus is too crude mechanically to be of practical service.

I believe that the following considerations will ultimately lead to a successful solution of the problem.

First. The desired dispersion may be obtained by the use of a large diffraction grating, mounted with a pair of telescopes of large aperture. The grating should be bright in the second or third order if a ruling with about fourteen thousand lines to the inch is used. Suitable absorbing media will be required before the slit to cut out the overlapping spectra. The spectroscope should be mounted upon an equatorial refracting telescope.

Secondly. It is a well known fact, that the solar line C is the best line for prominence observation because of the brilliant and well defined images seen through it. Therefore this line, in spite of its position at the red end of the spectrum, should be used in preference to any other for prominence photography. Ordinary dry plates are more sensitive to the region of the spectrum near F and $H\gamma$, but the nebulous character of these lines makes it more desirable to use C, though for sharpness even this line is not all that could be desired. Hence special plates, sensitive to red light, are required. D_3 is a very sharp line, and as it is

always visible as far above the limb as C, it is also recommended for photographic work, though the continuous spectrum at this point may prove troublesome. This line would of course require the use of plates sensitive to the yellow rays.

Thirdly. For photographing the prominences two new methods are offered. In general, they are as follows.

Method A. Change the rate of the driving clock of the telescope, so as to make the sun's limb move slowly across the slit of the spectroscope. Move the photographic plate in the plane of dispersion, and at right angles to the axis of the observing telescope; its velocity will depend upon the form of spectroscope employed. In this manner a series of images of the slit would be formed side by side, and merging into each other, thus building up an image of the prominence.

This method would allow the use of a narrow slit, thus greatly reducing the atmospheric light and increasing the sharpness of the photograph. A second slit should be placed immediately in front of the plate, and so nearly closed as to allow the line in use to pass. This would cut out the continuous spectrum on either side, and leave the photograph of the prominence in strong contrast on a clear background. In practice it is impossible to fully realize this, but still the second slit can be closed to such an extent as greatly to reduce the superfluous light. The use of a second slit is not in itself new, as it was employed by Huggins and others as an aid in eye observation, and by Lohse in his " rotating spectroscope." It is easy to see that a radial slit would probably prove most useful for this work. But there are at a given time only two positions on the sun's limb at which the slit may be radial when the direction of the sun's motion is at right angles to it. Thus, if the prominence did not happen to be at either of these points, it could not be photographed to the very best advantage. But it is not absolutely essential that the motion should be at right angles to the slit, and in this case a prominence could be photographed at any point on the limb. The chromosphere and all the prominences visible at one time could be taken upon one plate by reducing the diameter of the solar image until it becomes less than the length of the slit; the sun being then allowed to move across centrally.

Method B. This method is similar in principle to the above, but the result is effected in a different way, making a radial slit applicable to any portion of the limb. Provide the slit with a uniform motion across the axis of the collimator. Before the photographic plate arrange a second slit, and cause this to move with the first slit by means of a screw; both screws being provided with grooved heads for a cord, and

driven by a single clock. In this case the plate and sun's image must be stationary. The slit then moves across the prominence, and the second slit prevents the plate from fogging.

These are methods devised by me a year ago. Some recent changes and improvements will be mentioned after describing the work already accomplished.

Through the kindness of Prof. E. C. Pickering the fifteen-inch equatorial of the Harvard Observatory was placed at my disposal in November, 1889. But as Mr. George Clark considered the weight of my large spectroscope too great to be safely carried by the wooden tube of the equatorial, it was finally decided to adapt the spectroscope to the twelve-inch horizontal telescope, which Professor Pickering very kindly gave up entirely to my use, although it was at that time employed for photometric work. This instrument consists of a telescope tube with twelve-inch objective supported horizontally. Light from any body not too far from the meridian is reflected into the tube by an eighteen-inch silvered glass mirror. This rests upon an iron plate in a cradle, which can be rotated by a friction roller attached to a long rod leading into the eye end of the telescope, thus giving the motion in altitude. The axis of the telescope is at right angles to the meridian, and a screw at the back of the mirror gives the motion in azimuth. This screw is driven by an endless cord in connection with a clock placed convenient to the hands of the observer, so that its rate can be easily regulated by screwing up or down the friction disk of a centrifugal governor. As the mounting is alt-azimuth, the image of the sun cannot be kept stationary by the clock, and Method B could not therefore be tried with this telescope.

The spectroscope was constructed for me by Mr. J. A. Brashear, and has proved itself in every respect a most excellent instrument. The slit is made of glass-hardened steel, gilded to prevent rust, and opens equally in both directions from the centre, the width being read off from a graduated head. The whole slit-plate is provided with a screw motion across the end of the collimator, — a very convenient motion in prominence or spot observation, and also necessary in Method B. A simple device allows three spectra to be taken edge to edge on the same plate. The lenses of the collimator and observing telescope are exactly alike, about $3\frac{1}{4}$ inches aperture and $42\frac{1}{2}$ inches focus, and are made of Jena glass. The whole collimator tube can be moved by a screw through collars in the frame, and thus the slit can be easily brought to the focus of the large telescope without moving the whole instrument. The grating is one of the excellent rulings of Professor

Rowland, and is remarkably bright in one of the second spectra. It is ruled with 14,438 lines to the inch on a highly polished surface of speculum metal, and contains altogether more than 48,000 lines. It is mounted in a holder with adjusting screws at the top and side, and stray light is excluded by a cylindrical brass cover, provided with close fitting openings for the two telescopes. The circular plate upon which the grating-holder stands is divided on its edge to degrees for convenience in setting, and a rod connected with a tangent screw carries the slow motion to the eye end of the spectroscope; a quick motion is also supplied. Eye-pieces of various powers are used for observation, either with an adapter at the end of the tube, or at the side with a total reflecting prism when the sliding plate-holder is in position. In the apparatus as first used, a small plate-holder was held by a spring clip in a light frame of brass tubing arranged to slide with very little friction between V-shaped guides. After many experiments the steadiest motion that could be obtained was derived by a fine wire from the clock of the horizontal telescope. But this was at best unsatisfactory, and several improvements have recently been devised to overcome the difficulty. An adjustable slit, directly in front of the plate and very near its surface, was used as described in Method A. The whole spectroscope is supported in a strongly braced frame of steel tubing, and by means of a gear and either one of two pinions it is easily rotated about the axis of the collimator. This allows the slit to be made tangent or radial at any point on the sun's limb.

At first thought, it would seem that the horizontal telescope offers many advantages for spectroscopic work. As the tube is always retained in a fixed position, and rests on solid brick piers, apparatus of great weight may be attached to the eye end without causing sensible flexure of the tube. The spectroscope is thus always conveniently placed for observation, and work may be carried on with comfort even in the coldest weather, as the observer sits in a small house, which may be warmed with a stove if so desired. But these advantages are more than outweighed by the poor performance of the mirror when used for solar work. Not only is a large amount of diffuse light sent into the spectroscope, but the distortion of the mirror by the sun's heat soon changes a prominence into a shapeless mass when the diffuse light does not render it entirely invisible. An example of the extreme sensitiveness of the mirror to slight heating was recently given me by Mr. Clark. He said that, when it was first set up for night work, the heat radiated from the brick pier upon the back of the mirror so distorted it that no star image could be obtained. This was due to the unequal radiating

power of the different surfaces of the mirror, and the difficulty was at length partially overcome by covering the circumference and sides with bright sheet tin, and thus rendering their radiating power more nearly that of the silvered front surface. But it is obvious that the direct solar rays could not but have a far greater effect than the feeble radiation from the pier, and the result was that prominences could only rarely be seen at all, and even when seen they were so faint and badly defined as to be practically useless for photographic work. As I have already remarked, none of the equatorials of the Observatory were available for this work; so the best that could be done was to use the horizontal telescope on such days as anything at all could be seen with it.

Attempts were made to photograph through the F and Hγ lines with ordinary dry plates, through D_3 with plates dyed with erythrosine, and through C with plates dyed with cyanine and alizarine blue. In the first three cases, Method A was used with some degree of success. F and Hγ gave the general outlines of the prominences rising above the limb, and on one plate taken through F two prominences are shown. The line D_3 was photographed by the use of a tangential slit, the sun's image being maintained stationary by the slow motions. At the red end of the spectrum, the cyanine and alizarine plates were found to be hardly sensitive enough for the purpose. It is hoped that further experiments with these and other dyes will remove the difficulty.

The investigation is soon to be resumed, under much better auspices, at the Kenwood Physical Observatory. The large spectroscope already described will be attached to a long-focus twelve-inch equatorial refractor, built especially for spectroscopic work. The object glass of this instrument is by Brashear, and the mounting by Warner and Swasey. It is covered by a dome 26½ feet in diameter, giving ample room for any desired apparatus at the eye end. A new frictionless arrangement has been devised, which by a simple change gives either a stationary slit and moving plate, or a moving slit and stationary plate. It is also considered possible, that under certain circumstances two or more slits may be used before the plate, thus giving simultaneous photographs of the same prominence several lines at once, and allowing accurate comparisons of form to be made. Professor Young has suggested that the motion of the plate in Method A may be so changed as to correct the distortion of the prominence in the plane of dispersion. I mean also to try a rotating cylinder at the focus of the spectroscope, carrying on its circumference a strip of celluloid photographic film close behind

the stationary slit. This will probably give the greatest steadiness of motion obtainable, if used with a carefully made clepsydra, possibly of the form perfected by Dr. Draper.

The sun-spot minimum being now well past, it is hoped that prominences will be more frequent and more brilliant than was the case last winter, and the substitution of an equatorial for the horizontal telescope will certainly render the solution of the problem much less difficult.

KENWOOD PHYSICAL OBSERVATORY, CHICAGO,
October 24, 1890.

THE STUDY OF FOREIGN LANGUAGES IN TECHNOLOGICAL SCHOOLS.

By ALPHONSE N. VAN DAELL, Professor of Modern Languages.

SCIENCE is exacting, and does not allow him who wishes to gain distinction in her pursuit many diversions. For the student of physics it is a difficult matter to find the necessary hours for keeping abreast, not of the whole science, for that is out of the question, but even of its advance in one special direction. In chemistry, mathematics, and other fields, the outlook is quite similar. But the question of a programme of studies for a Technological School is not one of how best to suit the advanced scholar; it must be one of developing the qualities and perceptions most useful in a man of science. And even this is not enough, — "A man 's a man for a' that," — the proposed course must not only serve the more immediate purpose of the student, it must allow the growth of whatever befits the mind of a *man*.

In many cases, for which provision has been made in the Institute of Technology by the creation of a General Course, while science has remained a main object, the desire for general culture dominates. This one consideration would be sufficient for justifying the maintenance of a department of languages; but there is even a more intimate and deeper-lying reason for giving great importance to the study of languages. Close and accurate thinking is wellnigh impossible without adequate representation of human thought by words. Then how is the man of science to communicate the result of his studies to the world, if he cannot clothe his thoughts in words ? It is surely not saying too much, if we affirm that the world has been deprived of many useful devices because the inventors were not able to explain their discoveries. The greatest genius is incomplete and condemned to waste if he cannot draw others to him and make them understand and appreciate his value.

Language then is all important to every one. But why should foreign languages accompany the mother tongue? First, because French and German are in themselves a powerful aid to the study of English. Secondly, because exercises in translation furnish the best possible practice in teaching pupils how to express a stated thought in words of their

own, in giving them a discriminating idea of the exact value of expressions. Thirdly, because students of specialties need the broadening influence of foreign civilizations, and because the modern civilizations of France and Germany especially are important factors in the shaping of contemporary ideas. Fourthly, because many important scientific works are written in foreign languages, and an advanced student cannot keep abreast of the best devices unless he is enabled to make use of the ideas conveyed through the medium of other tongues.

The various uses of these languages to the student of technology will show what an ideal course ought to be, and what direction the efforts of the men who have charge of these courses ought to take. The course ought, as we have seen, to be generally broadening to the mind. This can be attained only by causing students to read some of the best literature, and, as a rule, that literature ought to be modern prose. It is a fact that we are much more readily made to appreciate things of our own time. The more remote a civilization from our own epoch, the harder it becomes for us to enter into it with any degree of sympathy; and the amount of interest, of sympathy, felt by the reader, is the measure of the civilizing influence exercised by the author. Poetry, and more especially German poetry, ought not to be excluded; yet the greater part of the time given to the study of any language should be consecrated to common prose, which is a necessary foundation. Much reading, and fluent reading, ought to be done from the outset. This plan has the great advantage of giving a degree of interest which abstract rules, or even rules applied in the Ollendorfian fashion to "the handsome waistcoat of the naughty butcher," can never attain; furthermore, it allows to the students constant practice in giving expression to their thoughts in good English, and it prepares them directly for the fourth use above mentioned, that is, the ready use, in their different specialties, of works written in foreign languages.

Two years only are allowed to either French or German at the Massachusetts Institute of Technology, with three hours per week of class-room work, and six hours of outside studying. It is supposed that students on entering have done the value of one year's work, either in French or German, and the regular course provides for three years' supplementary work; viz. one year to complete the language that has been offered at the entrance examination, and the two remaining years for the other language.

It is expected that during this time students will obtain a sufficient acquaintance with both French and German for all practical uses. It is thought that the French classes have come up to the requirements; for

German, the results have not been as thorough as could be desired. But a serious effort is now being made, which, it is confidently expected, will enable the classes to read scientific German at sight. All those who are acquainted with both languages will acknowledge that, on account of national and linguistic peculiarities, the latter task is much more difficult than the former.

But, after all, we ought perhaps to do much more which is not practicable within the given time. We ought to get an outline of the literature, enter into the study of comparative grammar, and give a speaking acquaintance with French and German. A knowledge of general literature from a foreign standpoint could not fail to be humanizing, a study of comparative grammar would give a more accurate idea of the structure of English, and in the cases of foreign travel, and of personal intercourse with workmen or employees of different nationalities, facility in speaking the foreign language would prove a desirable acquisition.

Taking more time during the course at the Institute is entirely out of the question. In most, I was going to say in all cases, the student's attention is called to such a variety of important professional topics that his receptive faculty is taxed to the utmost. What can then be done? I think that many preparatory schools should take upon themselves the task of preparing the students for the elementary work in both languages; if this should be realized to a certain extent, instead of dividing classes into sections of equal attainments, we could have some of them undertaking more advanced work. To require this of all students appears impracticable, because of the extreme difficulty, in some parts of the country, of finding competent instructors in modern languages. This is not to be wondered at, when we see some of our largest colleges intrusting the teaching of modern languages to instructors who take these positions as a temporary makeshift until some more congenial opening is offered, and when we hear in Boston itself instructors in preparatory schools openly proclaim, as if it were something to be proud of, that secondary schools need pay no attention to the advanced requirements of more progressive colleges. This is the language of blind prejudice; but, unfortunately, persons of that disposition are yet holding prominent and influential positions, although the number of these "laudatores temporis acti" is steadily diminishing.

There is a growing desire of becoming better acquainted with the masterpieces of foreign literature, and with the best thought of other nations. The old-fashioned way, to consider French or German as something good enough to fill spare hours, is fast disappearing. In the Massachusetts Institute of Technology we have now both for French

and German a four years' course, including the principles of grammar, the study of literature, and, when the pupils are advanced enough to hear a lesson given in the language and are capable of answering, the vernacular itself is taught.

Through the liberality of the Corporation, a library is now being started where students may find a series of selected reference-books which many could not afford to buy. It is to be hoped that this library may be increased from time to time, and the aid of friends would be welcome in that direction. A collection of general and technological dictionaries and vocabularies, and of the best editions of standard literatures, ought to find a place there.

The Massachusetts Institute of Technology has also offered courses in Italian and in Spanish; and it is the fond hope of the department that these may develop. Mexico presents a new and extensive field for young and enterprising engineers, and students of art and architecture ought to be given an opportunity for becoming acquainted with Italian art, so far as this can be done through the medium of the language.

THE APPLICATION OF CERTAIN DETERMINANT THEOREMS TO A PARTICULAR CASE.

BY H. W. TYLER, ASSISTANT PROFESSOR OF MATHEMATICS.

AMONG the various determinants occurring in algebra, perhaps none possesses greater interest than the determinant of order $2n$,

$$S = \begin{vmatrix} a_0 & a_1 & a_2 & \cdots & a_{n-1} & a_n & 0 & \cdots & 0 \\ 0 & a_0 & a_1 & \cdots & a_{n-2} & a_{n-1} & a_n & \cdots & 0 \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ 0 & 0 & 0 & \cdots & a_0 & a_1 & a_2 & \cdots & a_n \\ b_0 & b_1 & b_2 & \cdots & b_{n-1} & b_n & 0 & \cdots & 0 \\ 0 & b_0 & b_1 & \cdots & b_{n-2} & b_{n-1} & b_n & \cdots & 0 \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ 0 & 0 & 0 & \cdots & b_0 & b_1 & b_2 & \cdots & b_n \end{vmatrix} \tag{1}$$

The relations of this determinant to the two polynomials

$$\left.\begin{aligned} f(x) &= a_0 x^n + a_1 x^{n-1} + \cdots + a_{n-1} x + a_0 \\ \phi(x) &= b_0 x^n + b_1 x^{n-1} + \cdots + b_{n-1} x + b_0 \end{aligned}\right\} \tag{2}$$

and to the equations $f(x) = 0$ and $\phi(x) = 0$, are well known. We have, namely, the identity

$$S = A(x) \cdot f(x) + B(x) \cdot \phi(x),$$

where $A(x)$ and $B(x)$ are certain polynomials of the $(n-1)$st degree in x. Moreover if $f(x) = 0$ and $\phi(x) = 0$ have a common root, $S = 0$, and *vice versa*.

In this paper I shall, following a different direction from that just indicated, develop certain properties of the determinant S, primarily independent of its significance in general algebra.

GENERAL THEOREMS.

For convenience I may preface the treatment of the special problem by the needed theorems from the general theory of determinants. Since proofs of these theorems are for the most part accessible, they will not here be given;[1] subsequent applications may be easily verified individually.

[1] See Gordan's "Vorlesungen über Invariantentheorie," Vol. I.

For brevity, the matrix

$$M = \begin{vmatrix} s_{11} & s_{12} & \cdots & s_{1m} \\ s_{21} & s_{22} & \cdots & s_{2m} \\ s_{n1} & s_{n2} & \cdots & s_{nm} \end{vmatrix}$$

will be denoted by $|s_{ik}|$, the number of rows and columns being usually indicated by the context.

Definition I. The product of two matrices, $A = |a_{ik}|$ and $B = |b_{ik}|$, having each m rows and n columns, where $(n < m)$ is a determinant, $P = |p_{ik}|$, whose elements are derived from those of the two given matrices, as in multiplication of determinants, by the formula

$$p_{ik} = a_{1i}b_{1k} + a_{2i}b_{2k} + \cdots + a_{mi}b_{mk}. \tag{3}$$

Example:

$$\begin{vmatrix} a_{11} & a_{12} \\ a_{21} & a_{22} \\ a_{31} & a_{32} \end{vmatrix} \times \begin{vmatrix} b_{11} & b_{12} \\ b_{21} & b_{22} \\ b_{31} & b_{32} \end{vmatrix} = \begin{vmatrix} a_{11}b_{11} + a_{21}b_{21} + a_{31}b_{31} & a_{11}b_{12} + a_{21}b_{22} + a_{31}b_{32} \\ a_{12}b_{11} + a_{22}b_{21} + a_{32}b_{31} & a_{12}b_{12} + a_{22}b_{22} + a_{32}b_{32} \end{vmatrix}$$

Theorem I. If we denote by $M_{\bar{k}}$, $M_{\bar{i}}$, $M_{\bar{i}}$, respectively, the matrix obtained by omitting the k-th column of M, the i-th row, or both, and if

$$P = A \times B,$$

then is

$$P_{\bar{i}}^{\bar{k}} = A_{\bar{i}} \times B_{\bar{k}}.$$

Example:

$$\begin{vmatrix} a_{11} & a_{12} & a_{13} \\ a_{21} & a_{22} & a_{23} \\ a_{31} & a_{32} & a_{33} \end{vmatrix} \times \begin{vmatrix} b_{11} & b_{12} & b_{13} \\ b_{21} & b_{22} & b_{23} \\ b_{31} & b_{32} & b_{33} \end{vmatrix} = \begin{vmatrix} a_{11}b_{11} + a_{21}b_{21} + a_{31}b_{31} & a_{11}b_{12} + a_{21}b_{22} + a_{31}b_{32} \cdots \\ a_{12}b_{11} + a_{22}b_{21} + a_{32}b_{31} \cdots & \\ \cdots & \end{vmatrix}$$

Comparison with the previous example shows that

$$P_{\bar{3}}^{\bar{3}} = A_{\bar{3}} \times B_{\bar{3}}.$$

More generally, if $P = A \times B$,

$$P_{\substack{i_1 i_2 \cdots i_\mu \\ k_1 k_2 \cdots k_\mu}} = A_{\overline{i_1 i_2 \cdots i_\mu}} \times B_{\overline{k_1 k_2 \cdots k_\mu}}.$$

Definition II. If the elements of two matrices, $A = |a_{ik}|$ and $B = |b_{ik}|$, having each m rows and respectively n and $(m - n)$ columns, satisfy the relation

$$a_{1i}b_{1k} + a_{2i}b_{2k} + \cdots + a_{mi}b_{mk} = 0$$

for all pairs of values $i = 1, 2, \ldots n$; $k = 1, 2, \ldots (m - n)$; A and B are called *corresponding matrices.*

Example: $A = \begin{vmatrix} a_{11} & a_{12} \\ a_{21} & a_{22} \\ a_{31} & a_{32} \\ a_{41} & a_{42} \end{vmatrix}$ and $B = \begin{vmatrix} b_{11} & b_{12} \\ b_{21} & b_{22} \\ b_{31} & b_{32} \\ b_{41} & b_{42} \end{vmatrix}$

are corresponding matrices if

$$\begin{aligned} a_{11}b_{11} + a_{21}b_{21} + a_{31}b_{31} + a_{41}b_{41} &= 0 \\ a_{12}b_{11} + a_{22}b_{21} + a_{32}b_{31} + a_{42}b_{41} &= 0 \\ a_{11}b_{12} + a_{21}b_{22} + a_{31}b_{32} + a_{41}b_{42} &= 0 \\ a_{12}b_{12} + a_{22}b_{22} + a_{32}b_{32} + a_{42}b_{42} &= 0 \end{aligned}$$

Definition III. The omission of $(m-n)$ rows of A of Definition II. leads to a determinant of order n; this determinant is said to *correspond* to the determinant of order $(m-n)$, consisting of the similarly situated $(m-n)$ rows of the corresponding matrix B.

Example. Omission of 1st and 3d rows of A in previous example leaves $\begin{vmatrix} a_{21} & a_{22} \\ a_{41} & a_{42} \end{vmatrix}$; the corresponding determinant of B is $\begin{vmatrix} b_{11} & b_{12} \\ b_{31} & b_{34} \end{vmatrix}$.

Theorem II. Corresponding determinants of corresponding matrices are (in absolute value) proportional.

Theorem III. If A and B are corresponding matrices, and C any other matrix having the same number of rows and columns as A, then is

$$A \times C = \lambda \, | \, B \, C \, | \, ,$$

the expression $| \, B \, C \, |$ denoting the determinant composed of the columns of B and of C; λ is the ratio between corresponding determinants of A and B.

Example: If $\begin{vmatrix} a_{11} & a_{12} \\ a_{21} & a_{22} \\ a_{31} & a_{32} \end{vmatrix}$ and $\begin{vmatrix} b_{11} \\ b_{21} \\ b_{31} \end{vmatrix}$ correspond, then is

$$\begin{vmatrix} a_{11} & a_{12} \\ a_{21} & a_{22} \\ a_{31} & a_{32} \end{vmatrix} \times \begin{vmatrix} c_{11} & c_{12} \\ c_{21} & c_{22} \\ c_{31} & c_{32} \end{vmatrix} = \lambda \begin{vmatrix} b_{11} & c_{11} & c_{12} \\ b_{21} & c_{21} & c_{22} \\ b_{31} & c_{31} & c_{32} \end{vmatrix}, \text{ where } \lambda = \frac{\begin{vmatrix} a_{21} & a_{22} \\ a_{31} & a_{32} \end{vmatrix}}{b_{11}}$$

Theorem IV. Given n linear equations in m unknown quantities, where $m \gtrless n$,

$$\begin{aligned} A_1 &= a_{11}x_1 + a_{12}x_2 + \cdots + a_{1m}x_m = 0 \\ A_2 &= a_{21}x_1 + a_{22}x_2 + \cdots + a_{2m}x_m = 0 \\ &\cdots\cdots\cdots\cdots\cdots\cdots \\ A_n &= a_{n1}x_1 + a_{n2}x_2 + \cdots + a_{nm}x_m = 0 \end{aligned}$$

If we form by linear combination the n' equations

$$\begin{aligned}
B_1 &= \lambda_{11} A_1 + \lambda_{21} A_2 + \cdots + \lambda_{n1} A_n = b_{11} x_1 + b_{12} x_2 + \cdots + b_{1m} x_m = 0 \\
B_2 &= \lambda_{12} A_1 + \lambda_{22} A_2 + \cdots + \lambda_{n2} A_n = b_{21} x_1 + b_{22} x_2 + \cdots + b_{2m} x_m = 0 \\
&\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\
B_{n'} &= \lambda_{1n'} A_1 + \lambda_{2n'} A_2 + \cdots + \lambda_{nn'} A_n = b_{n'1} x_1 + b_{n'2} x_2 + \cdots + b_{n'm} x_m = 0
\end{aligned}$$

then is in general, by a simple extension of Definition I.,

$$B = \Delta \times A,$$

where $B = |b_{ik}|$, $A = |a_{ik}|$, and $\Delta = |\lambda_{ik}|$.

If, however, the quantities $x_{i_1} x_{i_2} \cdots x_{i_v}$ have been eliminated by the the linear combination, we have

$$B = \Delta \times A_{\overline{i_1 i_2 i_3 \cdots i_v}}. \tag{4}$$

If in particular $(n - n')$ unknowns are eliminated, then are, in general, Δ and the matrix of the omitted columns of A corresponding matrices.

Example:

$$\begin{aligned}
A_1 &= a_{11} x_1 + a_{12} x_2 + a_{13} x_3 = 0 \\
A_2 &= a_{21} x_1 + a_{22} x_2 + a_{23} x_3 = 0 \\
A_3 &= a_{31} x + a_{32} x_2 + a_{33} x_3 = 0
\end{aligned}$$

Let

$$\begin{aligned}
B_1 &= \lambda_{11} A_1 + \lambda_{21} A_2 + \lambda_{31} A_3 = b_{11} x_2 + b_{12} x_3 = 0 \\
B_2 &= \lambda_{12} A_1 + \lambda_{22} A_2 + \lambda_{32} A_3 = b_{21} x_2 + b_{22} x_3 = 0
\end{aligned}$$

then is

$$\begin{vmatrix} b_{11} & b_{12} \\ b_{21} & b_{22} \end{vmatrix} = \begin{vmatrix} \lambda_{11} & \lambda_{12} \\ \lambda_{21} & \lambda_{22} \\ \lambda_{31} & \lambda_{32} \end{vmatrix} \times \begin{vmatrix} a_{12} & a_{13} \\ a_{22} & a_{23} \\ a_{32} & a_{33} \end{vmatrix}$$

while, since x_1 has been eliminated, $\begin{vmatrix} \lambda_{11} & \lambda_{12} \\ \lambda_{21} & \lambda_{22} \\ \lambda_{31} & \lambda_{32} \end{vmatrix}$ and $\begin{vmatrix} a_{11} \\ a_{21} \\ a_{31} \end{vmatrix}$ are corresponding matrices.

Special Systems of Equations.

The general theorems above enunciated will now be applied to the determinant S, and to certain related matrices; for convenience, the case $n = 4$ will be considered.

The determinant

$$S = \begin{vmatrix} a_0 & a_1 & a_2 & a_3 & a_4 & 0 & 0 & 0 \\ 0 & a_0 & a_1 & a_2 & a_3 & a_4 & 0 & 0 \\ 0 & 0 & a_0 & a_1 & a_2 & a_3 & a_4 & 0 \\ 0 & 0 & 0 & a_0 & a_1 & a_2 & a_3 & a_4 \\ b_0 & b_1 & b_2 & b_3 & b_4 & 0 & 0 & 0 \\ 0 & b_0 & b_1 & b_2 & b_3 & b_4 & 0 & 0 \\ 0 & 0 & b_0 & b_1 & b_2 & b_3 & b_4 & 0 \\ 0 & 0 & 0 & b_0 & b_1 & b_2 & b_3 & b_4 \end{vmatrix}$$

is the coefficient-determinant of the equations

$$\left.\begin{aligned} x^3 \cdot f(x) &= a_0x^7 + a_1x^6 + a_2x^5 + a_3x^4 + a_4x^3 = 0 \\ x^2 \cdot f(x) &= a_0x^6 + a_1x^5 + a_2x^4 + a_3x^3 + a_4x^2 = 0 \\ x \cdot f(x) &= a_0x^5 + a_1x^4 + a_2x^3 + a_3x^2 + a_4x = 0 \\ f(x) &= a_0x^4 + a_1x^3 + a_2x^2 + a_3x + a_4 = 0 \\ x^3 \cdot \phi(x) &= b_0x^7 + \cdots + b_4x^3 = 0 \\ x^2 \cdot \phi(x) &= b_0x^6 + \cdots + b_4x^2 = 0 \\ x \cdot \phi(x) &= b_0x^5 + \cdots + b_4x = 0 \\ \phi(x) &= b_0x^4 + \cdots + b_4 = 0 \end{aligned}\right\} (5)$$

These eight equations may be regarded for our purposes as linear equations in the quantities $x^7, x^6, \ldots x, x^0$.

It was observed by Bézout in the last century, that certain linear combinations of these equations lack the terms x^7, x^6, x^5, x^4, as will now be shown.

Let $f(x) = a_0x^4 + a_1x^3 + a_2x^2 + a_3x + a_4$

$$\left.\begin{aligned} &= x^4(a_0) + (a_1x^3 + a_2x^2 + a_3x + a_4) = x^4A_0 + B_3 \\ &= x^3(a_0x + a_1) + (a_2x^2 + a_3x + a_4) = x^3A_1 + B_2 \\ &= x^2(a_0x^2 + a_1x + a_2) + (a_3x + a_4) = x^2A_2 + B_1 \\ &= x(a_0x^3 + a_1x^2 + a_2x + a_3) + a_4 = xA_3 + B_0 \end{aligned}\right\} (6)$$

and, similarly,

$$\phi(x) = x^4C_0 + D_3 = x^3C_1 + D_2 = x^2C_2 + D_1 = xC_3 + D_0, \qquad (6)$$

where the indices denote always the degree in x.

Now the equations

$$\left.\begin{aligned} -C_3f(x) + A_3\phi(x) &= 0 \\ -C_2f(x) + A_2\phi(x) &= 0 \\ -C_1f(x) + A_1\phi(x) &= 0 \\ -C_0f(x) + A_0\phi(x) &= 0 \end{aligned}\right\} (7)$$

are, on the one hand, linear combinations of the eight equations (5); on the other hand, they contain only the terms x^3, x^2, x, x^0.

Proof. The four equations may be written

$$\left.\begin{array}{rr} -b_0x^3\cdot f(x)-b_1x^2\cdot f(x)-b_2x\cdot f(x)-b_3f(x) & +a_0x^3\phi(x)+a_1x^2\cdot\phi(x)+a_2x\cdot\phi(x)+a_3\phi(x)=0 \\ -b_0x^2\cdot f(x)-b_1x\cdot f(x)-b_2f(x) & +a_0x^2\cdot\phi(x)+a_1x\cdot\phi(x)+a_2\phi(x)=0 \\ -b_0x\cdot f(x)-b_1f(x) & +a_0x\cdot\phi(x)+a_1\phi(x)=0 \\ -b_0f(x) & +a_0\phi(x)=0 \end{array}\right\} \quad (8)$$

On the other hand, replacing $f(x)$ and $\phi(x)$ in equations (7) by their values (6) in reverse order, we have

$$\left.\begin{array}{l} -C_3B_0+A_3D_0=0 \\ -C_2B_1+A_2D_1=0 \\ -C_1B_2+A_1D_2=0 \\ -C_0B_3+A_0D_3=0 \end{array}\right\} \quad (9)$$

and the indices show that none of these equations can be of higher degree than the third.

Similarly we may derive four equations containing only the powers x^7, x^6, x^5, x^4. Such are, namely, the equations

$$\left.\begin{array}{l} x^3[D_0f(x)-B_0\phi(x)]=0 \\ x^2[D_1f(x)-B_1\phi(x)]=0 \\ x\,[D_2f(x)-B_2\phi(x)]=0 \\ D_3f(x)-B_3\phi(x)]=0 \end{array}\right\} \quad (10)$$

for they may be written, on the one hand,

$$\left.\begin{array}{ll} b_4x^3\cdot f(x) & -a_4x^3\cdot\phi(x) \qquad\qquad\qquad\qquad\qquad\qquad =0 \\ b_3x^3\cdot f(x)+b_4x^2\cdot f(x) & -a_3x^3\cdot\phi(x)-a_4x^2\cdot\phi(x) \qquad\qquad\qquad =0 \\ b_2x^3\cdot f(x)+b_3x^2\cdot f(x)+b_4x\cdot f(x) & -a_2x^3\cdot\phi(x)-a_3x^2\cdot\phi(x)-a_4x\cdot\phi(x) \qquad =0 \\ b_1x^3\cdot f(x)+b_2x^2\cdot f(x)+b_3x\cdot f(x)+b_4f(x) & -a_1x^3\cdot\phi(x)-a_2x^2\cdot\phi(x)-a_3x\cdot\phi(x)-a_4\phi(x)=0 \end{array}\right\} \quad (11)$$

on the other hand, by means of (6),

$$\left.\begin{array}{l} x^4[-C_3B_0+A_3D_0]=0 \\ x^4[-C_2B_1+A_2D_1]=0 \\ x^4[-C_1B_2+A_1D_2]=0 \\ x^4[-C_0B_3+A_0D_3]=0 \end{array}\right\} \quad (12)$$

In order to obtain an analogous group of four equations containing any four consecutive powers of x, it is only necessary to combine the previous results. The equations containing x^4, x^3, x^2, x, are these:

$$\left.\begin{array}{l} D_0f(x)-B_0\phi(x)=0 \\ x[-C_2f(x)+A_2\phi(x)]=0 \\ x[-C_1f(x)+A_1\phi(x)]=0 \\ x[-C_0f(x)+A_0\phi(x)]=0 \end{array}\right\} \quad (13)$$

those containing x^5, x^4, x^3, x^2,

$$\left.\begin{aligned} x\,[D_0 f(x) - B_0\,\phi(x)] &= 0 \\ D_1 f(x) - B_1\,\phi(x) &= 0 \\ x^2\,[-C_1 f(x) + A_1\,\phi(x)] &= 0 \\ x\,[-C_0 f(x) + A_0\,\phi(x)] &= 0 \end{aligned}\right\} \tag{14}$$

and those containing x^6, x^5, x^4, x^3,

$$\left.\begin{aligned} x^2\,[D_0 f(x) - B_0\,\phi(x)] &= 0 \\ x\,[D_1 f(x) - B_1\,\phi(x)] &= 0 \\ D_2 f(x) - B_2\,\phi(x) &= 0 \\ x^3\,[-C_0 f(x) + A_0\,\phi(x)] &= 0 \end{aligned}\right\} \tag{15}$$

It appears by equations (9) and (12) that the five systems of equations (7), (10), (13), (14), and (15) differ from one another only in the exponents of x, and have thus all the same coefficient-determinant, which we shall denote by

$$R = |r_{i,k}|$$

Transition to Matrices.

From these related systems of equations the general theorems previously enunciated lead to corresponding relations between matrices. The equations (7) or (8) are derived, namely, by linear combination of the equations (5), the quantities x^7, x^6, x^5, and x^4 being thereby eliminated; applying the second part of Theorem IV., we have

$$R = \Delta \times S_{\overline{1234}};$$

that is,

$$R = \begin{vmatrix} -b_0 & 0 & 0 & 0 \\ -b_1 & -b_0 & 0 & 0 \\ -b_2 & -b_1 & -b_0 & 0 \\ -b_3 & -b_2 & -b_1 & -b_0 \\ a_0 & 0 & 0 & 0 \\ a_1 & a_0 & 0 & 0 \\ a_2 & a_1 & a_0 & 0 \\ a_3 & a_2 & a_1 & a_0 \end{vmatrix} \times \begin{vmatrix} a_4 & 0 & 0 & 0 \\ a_3 & a_4 & 0 & 0 \\ a_2 & a_3 & a_4 & 0 \\ a_1 & a_2 & a_3 & a_4 \\ b_4 & 0 & 0 & 0 \\ b_3 & b_4 & 0 & 0 \\ b_2 & b_3 & b_4 & 0 \\ b_1 & b_2 & b_3 & b_4 \end{vmatrix} \tag{16}$$

Similarly, the group (11) or (12), since x^8, x^2, x, x^0 have been eliminated, leads to the equation

$$R = \begin{vmatrix} b_4 & b_3 & b_2 & b_1 \\ 0 & b_4 & b_3 & b_2 \\ 0 & 0 & b_4 & b_3 \\ 0 & 0 & 0 & b_4 \\ -a_4 & -a_3 & -a_2 & -a_1 \\ 0 & -a_4 & -a_3 & -a_2 \\ 0 & 0 & -a_4 & -a_3 \\ 0 & 0 & 0 & -a_4 \end{vmatrix} \times \begin{vmatrix} a_0 & a_1 & a_2 & a_3 \\ 0 & a_0 & a_1 & a_2 \\ 0 & 0 & a_0 & a_1 \\ 0 & 0 & 0 & a_0 \\ b_0 & b_1 & b_2 & b_3 \\ 0 & b_0 & b_1 & b_2 \\ 0 & 0 & b_0 & b_1 \\ 0 & 0 & 0 & b_0 \end{vmatrix} \tag{16}$$

and, corresponding to the groups (13), (14), and (15), we have the intermediate products,

$$R = \begin{vmatrix} 0 & -b_0 & 0 & 0 \\ 0 & -b_1 & -b_0 & 0 \\ 0 & -b_2 & -b_1 & -b_0 \\ b_4 & 0 & 0 & 0 \\ 0 & a_0 & 0 & 0 \\ 0 & a_1 & a_0 & 0 \\ 0 & a_2 & a_1 & a_0 \\ -a_4 & 0 & 0 & 0 \end{vmatrix} \times \begin{vmatrix} a_3 & a_4 & 0 & 0 \\ a_2 & a_3 & a_4 & 0 \\ a_1 & a_2 & a_3 & a_4 \\ a_0 & a_1 & a_2 & a_3 \\ b_3 & b_4 & 0 & 0 \\ b_2 & b_3 & b_4 & 0 \\ b_1 & b_2 & b_3 & b_4 \\ b_0 & b_1 & b_2 & b_3 \end{vmatrix} = \begin{vmatrix} 0 & 0 & -b_0 & 0 \\ 0 & 0 & -b_1 & -b_0 \\ b_4 & b_3 & 0 & 0 \\ 0 & b_4 & 0 & 0 \\ 0 & 0 & a_0 & 0 \\ 0 & 0 & a_1 & a_0 \\ -a_4 & -a_3 & 0 & 0 \\ 0 & -a_4 & 0 & 0 \end{vmatrix} \times \begin{vmatrix} a_2 & a_3 & a_4 & 0 \\ a_1 & a_2 & a_3 & a_4 \\ a_0 & a_1 & a_2 & a_3 \\ 0 & a_0 & a_1 & a_2 \\ b_2 & b_3 & b_4 & 0 \\ b_1 & b_2 & b_3 & b_4 \\ b_0 & b_1 & b_2 & b_3 \\ 0 & b_0 & b_1 & b_2 \end{vmatrix} = \begin{vmatrix} 0 & 0 & 0 & -b_0 \\ b_4 & b_3 & b_2 & 0 \\ 0 & b_4 & b_3 & 0 \\ 0 & 0 & b_4 & 0 \\ 0 & 0 & 0 & a_0 \\ -a_4 & -a_3 & -a_2 & 0 \\ 0 & -a_4 & -a_3 & 0 \\ 0 & 0 & -a_4 & 0 \end{vmatrix} \times \begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ a_0 & a_1 & a_2 & a_3 \\ 0 & a_0 & a_1 & a_2 \\ 0 & 0 & a_0 & a_1 \\ b_1 & b_2 & b_3 & b_4 \\ b_0 & b_1 & b_2 & b_3 \\ 0 & b_0 & b_1 & b_2 \\ 0 & 0 & b_0 & b_1 \end{vmatrix} \tag{16}$$

These five product values of R are given in full, because on them depend the further results to be obtained. It will be observed that the second factor in every case consists of four consecutive columns of S. Moreover, by the last sentence in Theorem IV., it appears that in every case the matrix of the other four columns of S and the first factor are corresponding matrices. Such are, for example,

$$\begin{vmatrix} -b_0 & 0 & 0 & 0 \\ -b_1 & -b_0 & 0 & 0 \\ -b_2 & -b_1 & -b_0 & 0 \\ -b_3 & -b_2 & -b_1 & -b_0 \\ a_0 & 0 & 0 & 0 \\ a_1 & a_0 & 0 & 0 \\ a_2 & a_1 & a_0 & 0 \\ a_3 & a_2 & a_1 & a_0 \end{vmatrix} \quad \text{and} \quad S_{\overline{5678}} = \begin{vmatrix} a_0 & a_1 & a_2 & a_3 \\ 0 & a_0 & a_1 & a_2 \\ 0 & 0 & a_0 & a_1 \\ 0 & 0 & 0 & a_0 \\ b_0 & b_1 & b_2 & b_3 \\ 0 & b_0 & b_1 & b_2 \\ 0 & 0 & b_0 & b_1 \\ 0 & 0 & 0 & b_0 \end{vmatrix}$$

as may be readily verified by means of the equation of Definition II. These two corresponding matrices bear a very marked resemblance to each other. In fact, each arises out of the other by reversing the order of rows and of columns, and by changing the signs of the b's. With the corresponding matrices,

$$\begin{vmatrix} 0 & -b_0 & 0 & 0 \\ 0 & -b_1 & -b_0 & 0 \\ 0 & -b_2 & -b_1 & -b_0 \\ b_4 & 0 & 0 & 0 \\ 0 & a_0 & 0 & 0 \\ 0 & a_1 & a_0 & 0 \\ 0 & a_2 & a_1 & a_0 \\ -a_4 & 0 & 0 & 0 \end{vmatrix} \text{ and } \begin{vmatrix} a_0 & a_1 & a_2 & 0 \\ 0 & a_0 & a_1 & 0 \\ 0 & 0 & a_0 & 0 \\ 0 & 0 & 0 & a_4 \\ b_0 & b_1 & b_2 & 0 \\ 0 & b_0 & b_1 & 0 \\ 0 & 0 & b_0 & 0 \\ 0 & 0 & 0 & b_4 \end{vmatrix} = S_{\overline{4567}}$$

the mutual relation of form is somewhat more complicated, but still definite and intelligible, although not easy to formulate. We shall say that either matrix arises by inversion of the other, and shall use the process of inversion to derive a matrix corresponding to a given matrix having the form of one of our five factors, Δ.

The results thus attained may be summed up as follows.

Theorem V. To the matrix obtained by omitting any n consecutive columns of the determinant S of order $2n$ corresponds a matrix obtained by inversion of the one taken. The new matrix multiplied by the matrix of the n consecutive columns of S gives a determinant R of order n, which is in every case the same. The determinant R may thus be represented in $(n+1)$ ways as the product of two matrices, each of n columns and $2n$ rows.

Relations between R and S.

By means of the product values of R it will be proved:

1. That $R = S$.

2. That every first subdeterminant of R is equal to a second subdeterminant of S.

1. *Proof that* $R = S$. The first part of Theorem V. enables us to apply to any of the five product values of R in equations (16) Theorem III. We have, for example:

$$R = \begin{vmatrix} 0 & -b_0 & 0 & 0 \\ 0 & -b_1 & -b_0 & 0 \\ 0 & -b_2 & -b_1 & -b_0 \\ b_4 & 0 & 0 & 0 \\ 0 & a_0 & 0 & 0 \\ 0 & a_1 & a_0 & 0 \\ 0 & a_2 & a_1 & a_0 \\ -a_4 & 0 & 0 & 0 \end{vmatrix} \times \begin{vmatrix} a_3 & a_4 & 0 & 0 \\ a_2 & a_3 & a_4 & 0 \\ a_1 & a_2 & a_3 & a_4 \\ a_0 & a_1 & a_2 & a_3 \\ b_3 & b_4 & 0 & 0 \\ b_2 & b_3 & b_4 & 0 \\ b_1 & b_2 & b_3 & b_4 \\ b_0 & b_1 & b_2 & b_3 \end{vmatrix} = \begin{vmatrix} a_0 & a_1 & a_2 & 0 & a_3 & a_4 & 0 & 0 \\ 0 & a_0 & a_1 & 0 & a_2 & a_3 & a_4 & 0 \\ 0 & 0 & a_0 & 0 & a_1 & a_2 & a_3 & a_4 \\ 0 & 0 & 0 & a_4 & a_0 & a_1 & a_2 & a_3 \\ b_0 & b_1 & b_2 & 0 & b_3 & b_4 & 0 & 0 \\ 0 & b_0 & b_1 & 0 & b_2 & b_3 & b_4 & 0 \\ 0 & 0 & b_0 & 0 & b_1 & b_2 & b_3 & b_4 \\ 0 & 0 & 0 & b_4 & b_0 & b_1 & b_2 & b_3 \end{vmatrix} \quad (17)$$

λ being the ratio between corresponding determinants of the corresponding matrices

$$\begin{vmatrix} 0 & -b_0 & 0 & 0 \\ 0 & -b_1 & \cdots & \\ \cdot & \cdot & \cdot & \cdot \\ -a_4 & 0 & 0 & 0 \end{vmatrix} \text{ and } \begin{vmatrix} a_0 & a_1 & a_2 & 0 \\ 0 & a_0 & \cdots & \\ \cdot & \cdot & \cdot & \cdot \\ 0 & 0 & 0 & b_4 \end{vmatrix}.$$

We have
$$\lambda = \frac{\begin{vmatrix} 0 & a_0 & 0 & 0 \\ 0 & a_1 & a_0 & 0 \\ 0 & a_2 & a_1 & a_0 \\ -a_4 & 0 & 0 & 0 \end{vmatrix}}{\begin{vmatrix} a_0 & a_1 & a_2 & 0 \\ 0 & a_0 & a_1 & 0 \\ 0 & 0 & a_0 & 0 \\ 0 & 0 & 0 & a_4 \end{vmatrix}} = \frac{a_0^3 a_4}{a_0^3 a_4} = 1;$$

and since the determinant in (17) coincides with S on transferring the fourth column to the end, we have

$$R = S.$$

2. *First Subdeterminants of R.* Theorem I. and equations (16) enable us to apply the same principle as above to some of the subdeterminants of R.

Example. Theorem I. applied to the first of equations (16) gives

$$R_{\frac{1}{2}} = \begin{vmatrix} 0 & 0 & 0 \\ -b_0 & 0 & 0 \\ -b_1 & -b_0 & 0 \\ -b_2 & -b_1 & -b_0 \\ 0 & 0 & 0 \\ a_0 & 0 & 0 \\ a_1 & a_0 & 0 \\ a_2 & a_1 & a_0 \end{vmatrix} \times \begin{vmatrix} a_4 & 0 & 0 \\ a_3 & 0 & 0 \\ a_2 & a_4 & 0 \\ a_1 & a_3 & a_4 \\ b_4 & 0 & 0 \\ b_3 & 0 & 0 \\ b_2 & b_4 & 0 \\ b_1 & b_3 & b_4 \end{vmatrix}$$

The definition equation (3) shows that the first and fifth rows of ***both*** matrices have no effect on the product; that is,

$$R_{\frac{1}{2}} = \begin{vmatrix} -b_0 & 0 & 0 \\ -b_1 & -b_0 & 0 \\ -b_2 & -b_1 & -b_0 \\ a_0 & 0 & 0 \\ a_1 & a_0 & 0 \\ a_2 & a_1 & a_0 \end{vmatrix} \times \begin{vmatrix} a_3 & 0 & 0 \\ a_2 & a_4 & 0 \\ a_1 & a_3 & a_4 \\ b_3 & 0 & 0 \\ b_2 & b_4 & 0 \\ b_1 & b_3 & b_4 \end{vmatrix}$$

The first factor is of the same form as the original factor in R; therefore inversion leads to a corresponding matrix, and we have by Theorem III.:

$$R_{\frac{1}{2}} = \lambda \begin{vmatrix} a_0 & a_1 & a_2 & a_3 & 0 & 0 \\ 0 & a_0 & a_1 & a_2 & a_4 & 0 \\ 0 & 0 & a_0 & a_1 & a_3 & a_4 \\ b_0 & b_1 & b_2 & b_3 & 0 & 0 \\ 0 & b_0 & b_1 & b_2 & b_4 & 0 \\ 0 & 0 & b_0 & b_1 & b_3 & b_4 \end{vmatrix} \quad \text{where } \lambda = \frac{\begin{vmatrix} a_0 & 0 & 0 \\ a_1 & a_0 & 0 \\ a_2 & a_1 & a_0 \end{vmatrix}}{\begin{vmatrix} a_0 & a_1 & a_2 \\ 0 & a_0 & a_1 \\ 0 & 0 & a_0 \end{vmatrix}} = 1.$$

The new determinant may be obtained from S by omitting the fifth and eighth columns and the fourth and eighth rows, or by omitting the first and sixth columns and the first and fifth rows. That is,

$$R_{\frac{1}{2}} = S_{\frac{48}{58}} = S_{\frac{15}{16}}$$

In general for $R_{\frac{i}{k}}$ we have only to choose such a product formula from (16) that the omission of the i-th column from the first factor shall leave two rows of zeros; the omission of these rows and of the corresponding rows of the other matrix gives a product to which Theorem III. is applicable. The determinant finally obtained arises from S by omission of the first and fifth rows and the first column, or of the fourth and eighth rows and the eighth column, and, in each case, of a second column whose number depends upon i and k. All such determinants are contained in the matrix

$$S' = S_{\frac{15}{1}} = S_{\frac{48}{8}} = \begin{vmatrix} a_0 & a_1 & a_2 & a_3 & a_4 & 0 & 0 \\ 0 & a_0 & a_1 & a_2 & a_3 & a_4 & 0 \\ 0 & 0 & a_0 & a_1 & a_2 & a_3 & a_4 \\ b_0 & b_1 & b_2 & b_3 & b_4 & 0 & 0 \\ 0 & b_0 & b_1 & b_2 & b_3 & b_4 & 0 \\ 0 & 0 & b_0 & b_1 & b_2 & b_3 & b_4 \end{vmatrix}; \tag{18}$$

we have, for example,

$$R_{\frac{1}{2}} = S'_{\overline{5}}.$$

The above process leads in the general case to the formula

$$R_{\frac{i}{k}} = S'_{\overline{n-i+k}} \tag{19}$$

In view of the simplicity of method and complexity of detail of the general case, only the following additional example need be given. The third product value of R leads to the equations

$$R_{\overline{3}} = \begin{vmatrix} 0 & 0 & 0 \\ 0 & -b_0 & 0 \\ 0 & -b_1 & -b_0 \\ b_4 & 0 & 0 \\ 0 & 0 & 0 \\ 0 & a_0 & 0 \\ 0 & a_1 & a_0 \\ -a_4 & 0 & 0 \end{vmatrix} \times \begin{vmatrix} a_3 a_4 0 \\ a_2 a_3 0 \\ a_1 a_2 a_4 \\ a_0 a_1 a_3 \\ b_3 b_4 0 \\ b_2 b_3 0 \\ b_1 b_2 b_4 \\ b_0 b_1 b_3 \end{vmatrix} = \begin{vmatrix} 0 & -b_0 & 0 \\ 0 & -b_1 & -b_0 \\ b_4 & 0 & 0 \\ 0 & a_0 & 0 \\ 0 & a_1 & a_0 \\ -a_4 & 0 & 0 \end{vmatrix} \times \begin{vmatrix} a_2 a_3 0 \\ a_1 a_2 a_4 \\ a_0 a_1 a_3 \\ b_2 b_3 0 \\ b_1 b_2 b_4 \\ b_0 b_1 b_3 \end{vmatrix} = \lambda \begin{vmatrix} a_0 a_1 0 & a_2 a_3 0 \\ 0 a_0 0 & a_1 a_2 a_4 \\ 0 0 a_4 & a_0 a_1 a_3 \\ b_0 b_1 0 & b_2 b_3 0 \\ 0 b_0 0 & b_1 b_2 b_4 \\ 0 0 b_4 & b_0 b_1 b_3 \end{vmatrix} = \begin{vmatrix} a_0 a_1 a_2 a_3 0 0 \\ 0 a_0 a_1 a_2 a_4 0 \\ 0 0 a_0 a_1 a_3 a_4 \\ b_0 b_1 b_2 b_3 0 0 \\ 0 b_0 b_1 b_2 b_4 0 \\ 0 0 b_0 b_1 b_3 b_4 \end{vmatrix} = S_{\overline{3}}$$

in accordance with (19).

For $n = 4$, (19) leads to the equations

$$\begin{aligned}
R_{\underset{1}{4}} &= S'_{\overline{1}} \\
R_{\underset{1}{3}} = R_{\underset{2}{4}} &= S'_{\overline{2}} \\
R_{\underset{1}{2}} = R_{\underset{2}{3}} = R_{\underset{3}{4}} &= S'_{\overline{3}} \\
R_{\underset{1}{1}} = R_{\underset{2}{2}} = R_{\underset{3}{3}} = R_{\underset{4}{4}} &= S'_{\overline{4}} \\
R_{\underset{2}{1}} = R_{\underset{3}{2}} = R_{\underset{4}{3}} &= S'_{\overline{5}} \\
R_{\underset{3}{1}} = R_{\underset{4}{2}} &= S'_{\overline{6}} \\
R_{\underset{4}{1}} &= S'_{\overline{7}}
\end{aligned}$$

Theorem VI. The determinants R and S are equal. Each of the n^2 first subdeterminants of R is equal to one of those $(2n - 1)$ second subdeterminants of S contained in the matrix $S' = S_{\underset{1}{1},n+1} = S_{\underset{2n}{n},2n}$, as expressed by equation (19).

The same method may be applied to some of the second subdeterminants of R, but not to all. A treatment of this case, as well as proofs of the general theorems above used, is to be published in the *Sitzungsberichten der Erlanger physikal. medicin. Societät.* It may be added, that whatever is new in the present paper is largely due to the suggestions of Professor P. Gordan of Erlangen.

THE PRECIPITATION OF MANGANESE AS AMMONIUM MANGANOUS PHOSPHATE.

BY ALEXANDER G. McKENNA.

IN the determination of manganese in iron ores, it is customary to precipitate the manganese as dioxide, after separation from the sesquioxides by the basic acetate process, by adding bromine to the filtrate from the acetic acid precipitation. The manganese thus thrown down may be ignited and weighed as manganese proto-sesquioxide, but the results are inaccurate from two causes. Manganese dioxide when precipitated in the presence of sodium or potassium salts always carries down with it some alkali, which appears to be chemically combined with the manganese, since it cannot be removed by the most prolonged washing with water.[1]

This source of error can be avoided by using ammonium acetate in the basic separation, but in this case the solution must be made alkaline before manganese can be precipitated by bromine, and the separation from calcium is then rendered less sharp.

Another objection to the determination of manganese based on the ignition of the precipitated dioxide is the uncertainty of the resulting oxide, which is generally assumed to be Mn_3O_4. Pickering[2] has shown that the percentage of manganese in this oxide varied between 69.69% and 75%, according to the temperature of ignition and other causes.

It is now customary to redissolve the manganese dioxide in hydrochloric acid, and then precipitate it as ammonium manganous phosphate. The precipitate is ignited and weighed as manganese pyrophosphate. This method of determining manganese, which we owe to Dr. Wolcott Gibbs, is now generally admitted to be the most accurate as well as the most easily performed of all the methods of determining manganese. It has been variously modified by many chemists, but none of these modifications seem to possess any advantages over the original process as given by Dr. Gibbs.

The following work was undertaken at the suggestion of Dr. T. M. Drown, to determine if some of the proposed changes in the details

[1] See Wright and Menke on Manganese Dioxide, in the Journal of the Chemical Society for 1880, p. 23.

[2] Chem. News, Vol. XLIII. p. 226.

of the process were of any advantage in simplifying the method or enhancing its accuracy.

Two solutions of manganous chloride were prepared, and measured portions were taken for precipitation. In the first method, Gibbs's original directions were followed.[1] The precipitation was made in a platinum dish, by adding a solution of sodium hydrogen phosphate in excess. The white precipitate which formed was dissolved in hydrochloric acid, the solution heated to boiling, and ammonia added in excess. The solution was now boiled for ten minutes and allowed to stand near the boiling point for one hour. It was filtered hot, and washed with hot water.

In the second method, Blair's[2] directions were followed. The solution was made acid with hydrochloric acid, an excess of sodium hydrogen phosphate added, the solution brought to boiling in a platinum dish, and ammonia added drop by drop, with constant stirring, until further addition of a drop of ammonia made no change in the silky character of the precipitate; a dozen drops more of ammonia were added, and after boiling a minute longer the solution was cooled in ice water before filtering. The wash water used was made by dissolving ten grams of ammonium nitrate in 100 c.c. of slightly ammoniacal water.

The chief object in view in the third method was to discover if there is any valid objection to making the precipitation in glass, and also to determine the length of time required for complete precipitation. To the solution of manganese salt in a beaker, hydrochloric acid and sodium hydrogen phosphate were added. The solution was heated to boiling, and 20 c.c. ammonia water added. The boiling was continued three minutes, with constant stirring, which is absolutely necessary to prevent bumping. The wash water used was made by adding one volume of ammonia (sp. gr. 0.96) to three volumes of water. Following are the results obtained in this series of analyses.

[1] Am. Journal of Science [II.], Vol. XLIV. p. 216.
[2] The Chemical Analysis of Iron, p. 93.

COMPARISON OF RESULTS OBTAINED BY THE THREE METHODS.

	Gibbs's Original Method.	Blair's Method.	Precipitation in Glass and washing with Ammonia Water.
	Grams.	Grams.	Grams.
Weight of Manganese Pyrophosphate obtained, first Solution	.4990	.4997	.4994
	.4996	.4992	.5007
Do., second Solution	.0897	.0896	.0894

Three more determinations were made by precipitation in glass, but the precipitates did not become crystalline in three minutes, and the washings were turbid.

The manganese precipitates after ignition were examined for silica, but none was found.

This set of experiments seems to show that essentially the same results are obtained by following either Gibbs's or Blair's directions; also that glass is not attacked by the solution in three minutes' boiling, but that this time is not always sufficient to insure the complete conversion of the precipitate to the crystalline form which is necessary before filtration.

Another series of experiments was made in the same manner, but with a different amount of manganese.

SECOND SERIES OF RESULTS.

	Gibbs's Method.	Blair's Method.	In Glass as before.
	Grams.	Grams.	Grams.
Weight of Manganese Pyrophosphate obtained	.2415	.2432	.2412
	.2427	.2432	.2427
	.2408	.2385	.2425
	.2432	....[1]	.2387[1]
	.2355[1]	....[1]	

These results confirm those in the first series, and indicate the necessity of having the precipitate thoroughly crystalline.

[1] Not crystalline.

The amount of manganese in the filtrates and washings of each set of these experiments was determined, (except in those cases where the precipitate was not crystalline,) with the following results. The total volume of the filtrate and washings was in each case about 300 c.c.

SOLUBILITY OF THE PRECIPITATE.

	Gibbs's Method. Washed with Water.	Blair's Method. Washed with Dilute Ammonium Nitrate.	In Glass. Washed with Dilute Ammonia.
	Average Gram.	Average Gram.	Average Gram.
Filtrate contained Metallic Manganese	.0005	.0004	....
Washings contained Metallic Manganese	.0005	.0012	.0003

In order to determine the relative values of the three wash waters under precisely the same conditions, about three times the usual amount of manganese was precipitated by Gibbs's method, and the precipitate was divided into approximately equal parts by pouring alternately on three filters. The precipitate in each filter was washed thirty times, 7 c.c. of wash water being used each time.

The following results were obtained in two experiments.

SOLUBILITY OF PRECIPITATE. SECOND SERIES.

	In Hot Water.	In Ammonia (1 : 4).	In Ammonium Nitrate.
	Grams.	Grams.	Grams.
Washings contained Metallic Manganese	.0012	.0019	.0024
	.0018	.0016	.0014

These results show that, if there is any preference, it is in favor of hot water.

On washing with ammonia water the precipitate is changed in color to a dark gold, which change does not occur with either of the other wash waters.

Another set of nine determjnations was made in glass by boiling the solution, with constant stirring, for eight miuutes, or until the precipitate seen through the walls of the beaker had a peculiar silky appearance, resembling that of mercurous chloride when precipitated

from a mercuric salt by stannous chloride. The precipitate was washed with hot water.

RESULTS OF PRECIPITATION IN A BEAKER AND WASHING WITH HOT WATER.

	Grams.	Grams.	Grams.
Weight of Manganese Pyrophosphate obtained	.2450	.2445	.2450
	.2445	.2443	.2445
	.2440	.2443	.2442

No silica could be found in any of the precipitates after ignition.

In one experiment iron was present in small amount with the manganese. The precipitated phosphate had a beautiful gold color, and on being washed with dilute ammonia dissolved gradually in the wash water, giving a brown-colored filtrate. Continued washings showed no diminution in the amount dissolved. On standing exposed to the air for several hours, a brown precipitate settled out from the wash water. This would seem to indicate the formation of some double salt of manganese and iron, which is soluble in ammonia when an excess of an alkaline phosphate is not present.[1]

MASS. INSTITUTE OF TECHNOLOGY,
May, 1890.

[1] Since the above was written, there has appeared in the School of Mines Quarterly, Vol. XI. No. 4, a paper on the same subject, by Mr. R. C. Boyd, which confirms, in general, the results given above.

THE DATA OF FILTRATION. — II. *ON CRENOTHRIX KÜHNIANA (RABENHORST), ZOPF.*[1]

BY WILLIAM T. SEDGWICK, ASSOCIATE PROFESSOR OF BIOLOGY.

THE object of filtration is purification. This, partial or complete, is also ordinarily the result of filtration, whether it be effected through longer or shorter reaches of soil, as in the natural filtration of the ground waters into wells or springs, or through artificial sand filters worked either intermittently or continuously. There are some cases on record, however, in which the filtrate, or effluent, thus obtained, instead of issuing pure and clear, and remaining so, has speedily become contaminated with an unsightly and sometimes ill-smelling vegetation of an unusual form and highly peculiar character, namely, a luxuriant growth of the "Well-thread," *Crenothrix Kühniana* (Rab.), Zopf. A recent attack of this singular plant in the water service of Rotterdam, in Holland, and its presence in certain water supplies of Europe and America, serve to make it of special interest at the present time, particularly to those engaged in the study of the principles of filtration. Crenothrix is interesting to the biologist also. Its striking habit of depositing the oxide of iron in its highly developed sheaths, and especially its vital dependence upon certain salts of iron; its size, perhaps the largest, among the bacteria, the smallest of living things; its faculty of leaving its outgrown shell, or sheath, after the latter has become a hindrance; the history of its shifting fortunes for a recognized place among the lower fungi; its absence from water perfectly purified, and its occurrence in water imperfectly purified, by filtration; — all of these tend to surround Crenothrix with peculiarities which excite the interest of the investigator.

The immediate cause of the present paper is a recent Report upon this and other organisms living in the underground portions of the Rotterdam Water Works, — a report published by Professor H. De Vries of Amsterdam, the well known botanist, who has directed a biological investigation, ordered by the authorities of Rotterdam, into the causes of an appearance of Crenothrix in the drinking water of that city. But quite apart from this recent attack, Crenothrix deserves close attention

[1] This paper was read, in part, before the Society of Arts on October 9, 1890.

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for what it has done, and is still doing, elsewhere. Moreover, it is more than likely that the success or failure of the continuous filtration of drinking water may largely depend upon our ability to forestall the development of Crenothrix in the effluent. In any event, we shall do well to bring together the facts that we have already at hand concerning Crenothrix, inasmuch as these are widely scattered, and accessible only to specialists.

Since Ehrenberg, the first biologist to bring the achromatic objective to bear upon the micro-organisms, botanists have observed in various natural waters brownish thread-like vegetable forms of extremely simple structure. New genera and species were speedily formed to receive them, and even previous to 1850 two of these genera were well recognized and known as *Leptothrix* and *Hypheothrix*. The former was then described by Kützing as consisting of *very slender filaments, free, without branches and without sheaths,* the latter of *filaments, unbranched, but covered with sheaths.*

Our plant appears to have been first observed by Professor Julius Kühn in 1852, in a drain pipe leading from the reservoir of a garden in the vicinity of Bunzlau (Silesia), and was soon after named by Rabenhorst in his honor, *Leptothrix Kühniana.* Under this title it appeared in Rabenhorst's "Algæ of Saxony and Central Europe." In 1865, however, Rabenhorst briefly described it, in his "Fresh-water Algæ," as *Hypheothrix Kühniana,* as follows: "*Hypheothrix Kühniana* (*Leptothrix Kühniana,* Rabenhorst). Scattered Hypheothrix, interwoven either closely or loosely, contaminated and colored with the oxide of iron. Trichomes (filaments) slightly curved; palely æruginous; indistinctly articulated; in places interrupted; having thick sheaths, folded and colorless. Diameter of the filaments 0.0001–0.00012″. Habitat, in drain pipes. First discovered by J. Kühn."[1]

In March, 1870, Professor Ferdinand Cohn, of Breslau, received for microscopical examination samples of water from a well located in a district of the city notorious for typhoid fever. Aside from a not uncommon turbidity, these waters contained small brownish yellow masses floating about in considerable numbers in flocks from one to two millimeters in size. After some time these settled to the bottom and formed themselves into larger flocks, which were found to be composed of the colorless filaments of an alga. The examination of fresh samples of water from the same well, and of samples from other wells in and about Breslau, revealed the presence of the same organism, so that Cohn was convinced that it was "a form very widely

[1] Rabenhorst, Flora Europæa Algarum Aquæ dulcis, etc., Bd. II. p. 88, Leipzig, 1865.

represented in well waters." Moreover, since it appeared to belong to an entirely new genus and species, Cohn proposed to call it the *Brunnenfaden*, i. e. the "Well-thread," and, turning this into more technical language, he proposed to establish for it a new genus, *Crenothrix*. Finally, Cohn made a thorough study of the life history of Crenothrix, and, finding but one kind, in which there was also an abundant and peculiar spore formation, he proposed for it the specific name of *polyspora*. These facts, and many more, are recorded in his well known paper on Crenothrix.[1]

Crenothrix in the Berlin Water Supply.

In 1878 there appeared in the Tegel water supply of Berlin, Germany, a serious contamination in the shape of a flocculent yellowish-brown substance, which, when the water was allowed to stand, settled to the bottom of the vessel containing it. The investigation of this contamination was referred by the government of the city, through Professor Virchow, to Professor Oskar Brefeld and Dr. W. Zopf, whose reports[2] form an important contribution to our knowledge of Crenothrix, and are especially interesting as a study of the first "water calamity" of magnitude, in which the evil was definitively traced to this organism as a cause. Examination soon revealed the fact that the yellowish brown flocculent masses were composed chiefly of Crenothrix. This was especially abundant in the reservoir, though present in the water of all the wells from which the supply was derived, as well as in the service pipes in the city itself. The report was summed up as follows: "The principal part of the contamination is Crenothrix; which lives in the water as its natural soil, and, by means of invisible germs, is able to develop and multiply, thus producing visible masses." The water of the lake (Tegel) itself, from the surface and from various depths, was free from Crenothrix, as was also the superficial layer of the lake bottom; but in numerous wells in Berlin and elsewhere, in the reservoir at Charlottenburg and in other water works, as well as in unfiltered water from the river Spree, it was found in more or less abundance. Examinations of the soil showed that "the distribution of Crenothrix in the ground is not

[1] F. Cohn, Ueber den Brunnenfaden (*Crenothrix polyspora*), etc. Beiträge zur Biologie, Bd. I. pp. 117–131.

[2] O. Brefeld und W. Zopf, Bericht an den Hohen Magistrat der Stadt Berlin über die von ihnen angeführten Untersuchungen des Tegeler Wassers, Berlin, 1879. W. Zopf, Entwickelungsgeschichtliche Untersuchung der Crenothrix polyspora, Die Ursache der Berliner Wassercalamität, Berlin, 1879.

merely superficial; on the contrary, its germs are to be found in water taken from the most various depths." Accordingly, the conclusion was drawn that "there is no reason to suppose that water drawn from a considerable depth would remain free from this contamination, since the germs of Crenothrix, which inevitably have access to such water, must, in all probability, develop in such wells, reservoirs, or pipes its characteristic flocculent vegetation." Inasmuch as it appeared to be completely removed by filtration, it was, however, concluded that "a degree of clearness and purity which constitutes an effectual removal of the contamination" can be thus obtained.

In the course of his microscopical examinations — chemical analyses unfortunately are wanting — Zopf detected Crenothrix in the water supply of Halle, and this organism was in turn identified by Professor Kühn of Halle as the same which he had originally discovered in drain pipes at Gnadeborg, near Bunzlau, in 1852. This identification was further confirmed by comparison of the *Crenothrix polyspora* from Berlin and Halle with dried specimens of *Hypheothrix* (*Leptothrix*) *Kühniana* of Rabenhorst. Accordingly, in his paper upon Crenothrix (*op. cit.*), Zopf suggests that in future this plant be known as *Crenothrix Kühniana*, Rabenhorst. This name, together with that of Zopf, who, as stated, has identified *Crenothrix* with *Hypheothrix Kühniana*, Rab., is adopted by the editors of the latest edition of Rabenhorst's "Kryptogamen-Flora," in which it is described provisionally under the genus *Crenothrix*, Cohn, as *Crenothrix Kühniana* (Rabenhorst), Zopf.[1] It is also believed by Cohn and by Zopf that Radlkoper's *Palmellina floccu-losa*, detected in certain wells in Munich (Zeit. für Biologie, Bd. I.), is a special form of the same plant.

Distribution of Crenothrix.

In 1879 Zopf found that Crenothrix had been reported only in a single German province, namely, Silesia. Kühn, Cohn, and Kirchner had reported it in that province alone, and only from drains, wells, and springs. Brefeld and Zopf found it abundant also in Brandenburg and in Saxony. Professor Kühn found so much of it in the Halle water service that he recommended resort to new wells in a different locality as a source of the public supply. From these and other considerations, Zopf was led to suspect that Crenothrix would be found eventually to be much more common than had been supposed hitherto. To this opinion he was led, not only by the fact of its abundance in

[1] Kryptogamen-Flora, 2te Auflage, Bd. I. p. 67, Leipzig, 1884.

ground waters as far apart as those of Breslau, Berlin, and Halle, but also because of his discovery of Crenothrix in the unfiltered surface water of the river Spree. Zopf's suspicion of the wide distribution of this organism has been fully justified. It is now so well known and so much dreaded in Europe, that De Vries, in his recent paper, has called it "the pest of water works."

In the United States, Crenothrix is probably also common. It is only recently, however, that its presence has been widely recognized, and Wolle, in his "Fresh-water Algæ of the United States," published in 1887, refers to it as follows (p. 294): "We are not aware that the plant was recognized in this country until recently, 1886. It was found in somewhat limited quantity near the inlet to Lake Chatauqua, New York, whence part of the water supply to the city of Jamestown, N. Y., is taken." It appears to have been brought to his notice for the first time by Mr. George W. Rafter, who had detected it in abundance in the water supply of Jamestown, in 1886.[1] It was also observed by Mr. G. H. Parker, Biologist to the State Board of Health of Massachusetts, in 1887, in the water supplies of Brookline, Watertown, and other places in Massachusetts, and it is now known to occur commonly throughout the State.[2] In some cases it causes great annoyance (as, for example, in the Wayland water supply), while in other cases (as, for instance, in the Brookline supply), although it is present in small quantities, it does not appear to be noticed by the consumers of the water.

From the facts already reported it is clear that Crenothrix has usually appeared in ground waters, although it was detected by Zopf in the river Spree. Brefeld and Zopf also conclude that it is able to live in the earth itself, and is actually present in the ground even to a great depth (60 feet). So far as I am aware, there is no case on record in which Crenothrix has given trouble in any purely surface water supply. It was found by Kühn in drains: "I have found the plant in drain pipes in great quantities. It can, by itself alone, choke up drain pipes if these are too small in caliber in proportion to the volume of water. Drain pipes which run full for a long time are in danger of being choked by this plant if the quality of the water favors its development."[3] It was detected by Cohn in wells and springs; by Zopf in

[1] G. W. Rafter, On the Fresh-water Algæ, and their Relation to the Purity of Public Water Supplies. Trans. Am. Soc. Civil Engineers, Vol. XXI. p. 497, December, 1889.

[2] Nineteenth Annual Report, State Board of Health of Massachusetts, pp. 89–94, Boston, 1888.

[3] Quoted by Zopf, from a letter written to him by Professor Kühn. Zopf, *op. cit.*, p. 18.

wells, reservoirs of well water, and in one case in a river, but was absent from the Tegel Lake and from filtered Spree (river) water. It was discovered by Rafter in the Jamestown, N. Y., supply, which is derived partly from wells and partly from Chatauqua Lake through a conduit. This conduit is about two miles long, passes through a swampy region, and is probably not perfectly tight, as it is said to have been "loosely constructed." Its occurrence in Massachusetts is usually in water derived from wells or "filtering galleries," as, for example, in Brookline, Watertown, and Wayland.

In all of these cases we are obviously dealing with filtered water, since the waters of springs and wells are usually only surface waters naturally filtered. As stated above, however, Brefeld and Zopf were led to believe that filtration offers "an effectual removal of the contamination." This is probably true, so far as a removal of the unsightly flocculent masses is concerned. But inasmuch as the source of Crenothrix appears to be somehow or other connected with filtration, as is indicated by its almost exclusive occurrence in great quantities, in ground (i. e. in filtered) waters, it is likely that this remedy would be in some cases merely a temporary one, particularly as the germs of Crenothrix are small enough to pass through many filtering materials. On the other hand, it is not unreasonable to suppose that the filtration of a surface water free from Crenothrix — inasmuch as by this operation such a water is virtually converted, more or less completely, into a ground water — may, in a certain number of cases, so alter the original composition of the water that it can support a luxuriant vegetation of Crenothrix. It appears, moreover, that this condition, with disastrous consequences, has in fact already occurred in some cases; and since this involves an unforeseen obstacle in the theory and practice of sand filtration upon a large scale, it cannot be too carefully considered.

The most noteworthy case of this kind is that already referred to as having occurred in Rotterdam, Holland, in 1887, and later. It is fully reported in an admirable paper by Hugo De Vries, Professor of Botany in the University of Amsterdam.[1] What immediately follows is taken almost literally from De Vries's Report.

[1] H. De Vries, Die Pflanzen und Thiere in den dunkeln Räumen der Rotterdamer Wasserleitung. Bericht über die Biologischen Untersuchungen der Crenothrix-Commission zu Rotterdam, vom Jahre 1887, pp. 1–55, Jena, 1890.

CRENOTHRIX IN THE ROTTERDAM WATER SUPPLY.

The Water Works of Rotterdam draw their water from the Maas, and clarify it by means of sand filtration after most of the sediment of the river has settled on standing.

Since their establishment, in 1874, these works have furnished a clear and sufficiently satisfactory water, until suddenly, in the spring of 1887, the so much dreaded *Crenothrix Kühniana* appeared in great quantities. This unfortunate phenomenon coincided with a considerable enlargement of the whole service. In 1886 thirteen new filters were made ready and put into use, having sixteen thousand square meters of surface, while at the same time the eight old ones, having an area of eight thousand square meters, were disconnected.

The building of a new engine-house was in progress, but the new reservoir for filtered water was not yet ready, and the filtered water from the new filters had meantime to be conducted through the old conduits and the old reservoir to the old pumps. This extensive enlargement of the establishment was a consequence of the annually increasing consumption of water in the city. The original establishment (1874) was based on an estimate of a daily consumption of at most five thousand cubic meters. In 1887 the daily consumption had reached a maximum of forty-three thousand cubic meters, and in 1889, of about fifty thousand. For several years previous, the management had labored under an excessive demand, which neither allowed the river water the requisite time for depositing its sediment sufficiently, nor permitted the basins and conduits to be cleaned properly from time to time. The attack of Crenothrix referred to above was as unexpected as it was sudden and severe, and the behavior of this bacterium in the water supplies of other cities aroused the fear that it would soon attain a far greater development, and would affect the purity of the water.

In order to avoid this danger, as far as possible, in the summer of the same year in which the Crenothrix had appeared in a threatening manner, a Commission was appointed by the city authorities, and requested to investigate scientifically the cause of the phenomenon, and, if possible, to recommend methods which should prevent an increase of the evil, and finally secure complete immunity from it. This Commission worked under circumstances which facilitated to an unusual degree a thorough scientific study of the problem. The impossibility referred to of cleaning all basins and conduits in a suitable manner had allowed the vegetation in them to develop to an extent

seldom witnessed by investigators. At the same time, the new filters and conduits lately put into operation served for comparison.

After the work of the Commission in the winter of 1887 and 1888 had been finished, several questions still remained whose solution was desired for the completeness of the study. The continuation of the works on the new building, and especially the cleaning of the old basins and conduits, in 1889, afforded this opportunity.

For the microscopical investigations, which were conducted by Professor De Vries and Dr. Dupont, a little laboratory was fitted up in the water tower which supports the high reservoir, and in the basement of which is the reservoir of unfiltered water. This laboratory enabled them to investigate at once, for the most part, the samples taken from the basins and canals, and to observe the remarkable plant and animal life of the underground conduits, etc. in full activity.

After these introductory observations De Vries divides that portion of his report which deals with Crenothrix into seven sections, which well illustrate the scope of his paper, as follows: —

1. Crenothrix Kühniana, the Pest of Water Works.
2. The Appearance of Crenothrix in the City Water Works in 1887.
3. The Vegetation in the Maas and in the Open Basins (of Unfiltered Water).
4. Investigation of the covered Conduits of Unfiltered Water.
5. Biological Considerations concerning the Life in dark Reservoirs.
6. Crenothrix in the Reservoirs of Filtered Water.
7. The Danger of Contamination of the Water before Filtration.

The first section affords a convenient summary of our knowledge of the life history of Crenothrix as worked out by Cohn, Zopf, and, for a closely related form, still more recently by Winogradsky.[1] It will be observed that Crenothrix is no longer considered to be an alga, but, instead, a fungus (Schizomycete), or one of the bacteria. Besides, it is included by Winogradsky as one among several giant bacteria which agree in a peculiar habit of acquiring incrustations of ferric oxide in or upon their sheaths. This habit of Crenothrix and other forms was observed many years ago; and Zopf was led to believe that the existence of the plant is absolutely dependent upon the presence of certain salts of iron. In this he has been confirmed by

[1] S. Winogradsky, Ueber Eisenbacterien. Botanische Zeitung, Bd. XLVI. (1888), p. 260.

Winogradsky, whose designation of such forms as the "iron bacteria" is certainly convenient.

De Vries continues as follows.

The Life History of Crenothrix.

The iron bacteria are the largest forms in the whole group of bacteria. Although their cells are invisible to the naked eye, they are so united, end to end, as to form threads, or filaments, which in turn tend to make "flocks," or masses visible to the naked eye, often a millimeter and sometimes a centimeter or more in size.

A good hand lens reveals the structure of the flocks very clearly, but for a complete recognition of the character of the organisms a microscopical examination is required. This shows that the cells composing the filaments have, for the most part, a cylindrical form. The whole filament is enveloped by a sheath which is at first thin, but gradually increases in thickness. The most striking and the most characteristic property of the iron bacteria is their power of depositing in this sheath the oxide of iron (ferric oxide). Entirely colorless and transparent at first, they gradually become pale yellow, and then darker, until finally they are deep brown. At the same time their consistency is altered. They are at first flexible and soft, but gradually become hard and brittle. The thick, hard, dark brown sheaths are then no longer adapted to the life of the enclosed cells, and sooner or later are abandoned by them. The cells push themselves in single file out of the end of the sheath, or sometimes, partly sidewise, through chinks in the sheath. In the latter case, the thread appears to branch; in the former, the escaped bacteria continue the old sheath, and the new parts speedily undergo the same transformation as the old, whereupon these also are in their turn forsaken. In this way, long and occasionally branched sheaths are formed by a comparatively few cells, and the older, lower cells in the filaments composing the flocks are thus found lifeless and empty, with only their tips still living. A comparatively slow increase of the bacteria may thus be accompanied by a relatively large sheath formation.

These organisms form their ferric oxide from the dissolved iron salts of the water, and especially from the most widely distributed of these compounds, the ferrous carbonate. Without soluble ferrous salts, their life cannot long continue. They thrive best in those waters which contain iron rust in the presence of organic matter, since in such waters the formation of ferrous carbonate is incessant. In all

probability, the oxidation of the ferrous to the ferric oxide forms an important part of the vital processes of these cells, and the brown substance deposited in the sheaths is to be regarded, not as the refined product, but as the dross of the operation. In colorless sheaths which had been abandoned, in spots, by their cells, Winogradsky observed that the yellow coloration occurred only where the cells lay. Around the empty spots, the sheath contained no ferric oxide. Equally ineffective are empty sheaths, or such as contain only dead cells.

Soluble ferrous salts form, therefore, an indispensable condition for the welfare of these organisms. In other respects, however, they are very easily satisfied. They are nourished partly by the ordinary salts dissolved in water, and partly by dissolved organic substances which originate in the putrefaction of vegetable and animal remains. On the other hand, traces of such compounds appear to suffice for them. Winogradsky cultivated them in a water to which only 0.005 to 0.01 per cent of calcium butyrate or sodium acetate had been added. Fortunately, the normal content of our drinking waters in organic matters is insufficient for their support; and when such waters have been freed by filtration from all suspended and decomposing particles, they no longer form a suitable nutritive medium for these, the so much dreaded enemies of water works. Most natural waters, however, before filtration, are more or less favorable for the support of these organisms.

Among the iron bacteria, *Crenothrix Kühniana*, or *Crenothrix polyspora*, the only species of the genus, is the commonest and the most important form found in water works. It appears to be a plant widely distributed throughout the world, growing chiefly or by preference in drain pipes, deep wells, and other badly lighted water chambers. It is due to its power of extraordinarily rapid increase that it often covers, in a few months after the introduction of its germs, or after the conditions for its nourishment have become favorable, the entire walls of the reservoir, of the drain pipes or conduits, with a felt as thick as a finger, of which single portions are soon sufficiently developed to be swept along with every current as brown or blackish flocks. In this way it comes to pass that it is a real calamity in water works, making the water disagreeable for domestic use, and entirely unfit for many industrial purposes.

In order to comprehend more fully how such a sudden increase takes place, we must consider somewhat more closely the structure and the development of Crenothrix. Both have been extensively and

thoroughly studied by Dr. W. Zopf, when the plant for the first time appeared in alarming quantities in a water supply. This was the case, as is well known, in Berlin, in 1878.

The first method of multiplication depends upon the capacity of all the cells composing a filament, if they become isolated in any way, to develop each a new filament. When, for instance, the cells free themselves from their old rust sheath, and, before they form a new envelope, are broken apart by the current or by a blow, obviously we have supplied the germ for one or several flocks. But besides this, the plant possesses other methods of multiplication. In the thicker filaments the cells divide not merely transversely, as in the thinner threads, but also lengthwise. Thereupon each cell divides into two or four adjoining cells. These latter separate and form the so-called microspores. In a filament in which this process has occurred, as soon as the cells have left their sheath in the usual manner, these microspores escape as innumerable entirely distinct masses, which speedily become scattered in the water. Each microspore, immediately after its escape, begins to divide. Division takes place in all directions, and a little cluster soon arises, composed of hundreds of cells. The sheath with which each cell envelopes itself changes in this case into a "slime" or jelly, which, however, becomes colored in the usual manner with the oxide of iron. By means of this "slime" the whole mass is stuck or pasted to any part of the walls of the reservoir, or to any objects within it. After some time the course of development changes, and each cell of the cluster grows out into a filament. In some of these the cells divide transversely only, and form the so-called macrospores; in others the above described microspores arise.

The slime clusters, after they have developed into bundles of filaments, are the well known "flocks" of Crenothrix. These latter arise, however, more quickly in other ways far more favorable for their dislodgment. This occurs, first, when the microspores do not leave their sheath early enough, or when in this process some of the spores are left behind in the sheath. They then germinate inside the latter, and each, growing out into a filament, bursts through the old envelope. A whole bundle of such new threads can thus be connected, at their point of origin, by the old sheath. Secondly, there is often observed an apparent branching in such a way that innumerable cells have stuck fast upon an old sheath, and there germinated. Flocks of this sort formed the bulk of this contamination in the Rotterdam Water Works. This sort of branching may be repeated so often that many flocks reach the size of a centimeter or more, as already stated. It is obvious that such

masses may easily be broken off from their brittle stalks, — the old dead sheaths, — and so join the current of water.

Every cell may, therefore, by successive divisions, develop a new flock. If foods are present in sufficient quantity, the divisions follow one another rapidly. Moreover, each little filament forms, again and again, new sheaths. From the most insignificant beginnings, a very extensive contamination of the water may thus occur within a short time. If we reflect, furthermore, how dilute are the solutions from which this plant is able to manufacture its foods, and that it lives quite as well in total darkness as in the daylight, it becomes clear that by all its peculiarities it is stamped as a veritable pest of water works.

The foregoing summary of the life history of Crenothrix by De Vries is based upon the observations of Cohn, Zopf, and Winogradsky. Winter, in his systematic treatment of the Fungi,[1] places Crenothrix provisionally at the end of the Schizomycetes, and gives the following brief characteristics of the genus: —

"*Crenothrix*, COHN.

"Filaments cylindrical, transversely divided into cells, slightly thickened and club-shaped above, sheathed. Increase takes place through the cells of which the filament is composed, which escape from the sheath and develop into new filaments. Reproduction by spores which are formed within the sheath through further division of the cells of the filaments. The spores either grow out into filaments directly, or through continued division form gelatinous colonies which later develop the filaments."

The species is characterized by Winter as follows: —

"*Crenothrix Kühniana* (Rabh.), ZOPF.

"Filaments occurring in whitish or brownish tufts, 1½–5 μ thick, at the end widened to 6–9 μ; cells of the filaments of very unequal length. Spores 1–6 μ in diameter. In wells, drains, etc.

"An often very troublesome fungus, since it contaminates water and chokes small pipes. The cylindrical filaments, slightly thickened and club-shaped above, are distinctly divided transversely into cells; these afterward separate from one another, but even in that case soon become enveloped in a sheath which, colorless at the start, becomes yellow or yellow-brown through the deposit of iron. The sheaths, at first closed, are eventually burst open by the growing cells within, which pass out.

[1] Rabenhorst's Kryptogamen-Flora, *loc. cit.*

Every individual cell can develop a new filament. In other cases the filament remains enclosed in its sheath; its cells subdivide into short disks, which by vertical divisions break up into small ball-shaped cells. These may be considered to be the spores of the organism. Even within the sheaths they often develop into new filaments, which penetrate the jelly-like swollen sheath, or they may leave the sheath and develop entirely without it; either growing out into filaments, or by continued division forming larger or smaller colonies of roundish cells, which are held together by the gelatinized membrane. These colonies are known as the Palmella form [*Zoöglœa*, Cohn], and are probably equivalent to *Palmellina flocculosa*, Radlkofer. Each of these cells, again, is able to form a new filament."

Appearance of Crenothrix in the Rotterdam Water Service.

After the Berlin water calamity had given the incentive, Crenothrix was sought for in the water supplies of other cities, and in 1882 was observed in Amsterdam, and the next year in Rotterdam, in insignificant quantities, however, and only on certain days of the year. Since that time it has increased regularly in Amsterdam, although not to such an extent that the inhabitants have made much complaint. In Rotterdam it remained very scarce until the spring of 1887, although it was to be found every year in the water supply during the hot months; but during the spring of 1887 numerous complaints came to the management of the water works because of the impurity of the water, and the Director thoroughly demonstrated the presence of considerable quantities of Crenothrix, as well in the service pipes of the city as in the conduits and reservoirs of the works. The evil increased perceptibly with the increasing warmth of summer, and in June of the same year led to the appointment of the Commission of Investigation mentioned above.

The first task before the Commission was to convince itself of the general distribution of the iron bacteria in the service pipes of the city. For that purpose water was drawn from the mains through the hydrants at twelve places as far as possible from each other. The result was, that by thorough flushing a brown sediment was forced out of the pipes, which had accumulated particularly in the so-called "dead ends." After the removal of this sediment, and after further daily flushing of the portion of the pipes in question, the water could clearly contain only the flocks carried directly with it, but even in this case there was often a not inconsiderable quantity of these. The microscopical exami-

nation of this deposit showed that it was composed for the most part of the filaments and sheaths of Crenothrix. The filaments were to be found in great quantities, alive, in all stages and forms of development; the sheaths were, in general, empty and abandoned, often broken to pieces or changed into a slimy mass filled with granules of ferric oxide, and given over to putrefaction.

Putrefactive bacteria and infusoria, in enormous quantities, dwelt in this substratum. Besides these, remains of plant and animal origin were found, which indicated with much probability a contamination of the filtered water from other sources, and furnished the first foothold for the investigation of the water works.

As was to be expected, the brown flocks, which, without being deposited, were carried along in the water, consisted for the most part of Crenothrix, accompanied here, as in the sediment, by another iron bacterium, *Cladothrix dichotoma*. Since these species, however, occur together in nature, as well as in other water works than these, this phenomenon is nothing remarkable.

There can be no doubt that the flocks borne along in the service pipes continue to grow. In the flowing water they clearly find no time to increase perceptibly, but in the deposits, in places where the water is at rest, this may take place. Whether the Crenothrix had fastened itself to the inner walls of the pipes and there increased to a harmful extent was not learned, although it was by no means unlikely.

The problem was narrowed somewhat by the following consideration. An extensive increase of Crenothrix can obviously only take place where, on the one hand, the spores of this plant exist, and on the other, where the necessary food materials are present. Now the water of the Maas contains a small amount of organic substances in solution, but this quantity is much too small for the nourishment of iron bacteria. Without the addition of organic compounds they thrive no better in the unfiltered water of the Maas, than in the filtered water of other rivers. The suspended detritus of vegetable particles offers to them, however, in unfiltered water, a source of nourishment, and fallen leaves, dead water plants, and remains of water animals, are for them the most favorable soil.

Apart from the question whether any one will ever succeed in making a sand filtration so perfect that never, from time to time, in certain places, can bacteria and their germs slip through, we can at any rate assume that by proper filtration the most important sources of nourishment for Crenothrix will be withheld, and that the filtered water will not be able to support it. This idea is confirmed partly by a con-

sideration of the second water supply of Rotterdam, partly by the experiments conducted in Berlin.

Delftshaven originally had its own water works. In 1886 it was absorbed by Rotterdam, and its small filtering establishment, operating only two filters, was brought under the direction of the Rotterdam Water Works. The water is drawn from the same source, namely, the Maas, and is therefore exposed to the same sources of contamination. Nevertheless, Crenothrix has never been observed in its service pipes, and the commission did not succeed in finding it there, in spite of careful investigations. It follows from this that filtered Maas water can be kept uncontaminated by this pest, even in the case of filters which have been operated for years.

Again, the Berlin "water calamity" of 1878 has been completely overcome by the sand filtration instituted at the Lake Tegel Water Works in 1883. Even during the following year (1884) a considerable improvement was observed, which was explained by a gradual flushing of the pipes freer from the previous accumulations of Crenothrix. In 1886, the iron bacteria appear to have entirely disappeared from the service. In 1885, they were still observed in certain places in the city, but never since. It was accordingly assumed that after proper filtration the brown flocks would disappear from the Rotterdam system. A thorough investigation of the condition of the service pipes was therefore not considered necessary.

From another point of view, the Commission felt at liberty to limit its investigations. Brefeld and Zopf had concluded from their study of earth samples, as recorded above, that Crenothrix grows "widely distributed in the water of the earth." They arrived at this conclusion from microscopial examination of the material obtained from borings carried to the depth of thirty to sixty feet, at the Tegel Water Works. Since active green algæ are reported by Zopf to have been seen in the same materials with the Crenothrix, and these obviously could not have lived long in the dark, the suspicion arises that these algæ, and perhaps Crenothrix also, must have come through fissures, or otherwise, from the neighboring wells, or from other bodies of water more or less exposed to light. Accordingly, the Commission repeated these experiments in the vicinity of the Rotterdam Water Works, but although many samples of earth from various depths were examined, no Crenothrix was found except in cases where there was obvious entrance from basins containing Crenothrix and algæ. They conclude, therefore, contrary to Zopf, that Crenothrix does not grow in the earth.

Having thus narrowed the inquiry by excluding the service pipes,

in consideration of the experience of Delftshaven and Berlin, and also excluding the soil by direct observation of the absence of Crenothrix in the earth, at least in the vicinity of the Rotterdam works, there remained to be investigated only (1) the water of Maas and the open settling basins; (2) the conduits of unfiltered water leading from the settling basins to the filters; (3) the filters themselves; and (4) the reservoirs, etc. of filtered water.

De Vries arranges the results of these investigations in three groups, of which the first deals with the organisms in the Maas and in the settling basins, the second with the conduits of unfiltered water, and the third with the conduits, reservoirs, etc. of filtered water. Space permits of but a brief summary of the results included in the first two groups; the third division will be more fully reported.

Crenothrix in the Unfiltered Water of Rotterdam.

De Vries reports that Crenothrix occurs by no means rarely in the Maas itself, along with other iron bacteria, sometimes detached, but more often fixed upon different water plants (*Potamogeton*, *Ceratophyllum*), and upon decaying leaves. By straining the water at the intake, direct proof was obtained that Crenothrix passed freely into the conduit leading to the settling basins. These are three in number, and have an aggregate area of 40,000 square meters. From 1880 to 1888 they were never cleaned, owing to the excessive water consumption in the city; and in 1887 they contained a rich growth of water plants, among which *Elodea Canadensis* was most conspicuous. Besides these, there were abundant diatoms, rotifers, etc., mussels, sponges, bryozoa, and Cordylophora; these were especially abundant near the outlet of the settling basins.

Crenothrix was everywhere, but especially near the outlet. Obviously it lived luxuriantly upon the decomposition products of the vegetation of the settling basins. The unclean condition of the basins also plainly favored its growth and increase, and was thus the first step in the serious contamination of the water supply.

From the settling basins the still unfiltered water passes into a large subterranean conduit, 185 meters long and about 3 meters high, from which it is pumped through a cast-iron pipe into a reservoir in the basement of the water tower. From this it flows by gravity to the filters, first through cast-iron pipes, and then through a conduit 350 meters long, which is tapped by the pipes passing to the several filters. This long journey which the unfiltered water must make in

total darkness forms one of the most interesting subjects of the investigation. With the exception of the iron pipes, the entire length was repeatedly visited and inspected. The conduit from the river to the settling basins was also visited, but not until 1889.

Almost everywhere the walls of these subterranean passages were found covered with living organisms, forming an almost uninterrupted lining of varying composition. The quieter portions of the reservoir, particularly, showed a growth to be compared only with a magnificent aquarium. Handsome patches of fresh-water sponges were everywhere covering the walls as a thin layer, and seldom forming the familiar finger-like forms. Many of the patches were more than a foot broad, but most of them were smaller. These sponges, moreover, were colorless, not green, as in the light. Between the sponge masses innumerable mussels were attached to the walls by their byssus threads. Most striking of all was the fresh-water polyp (*Cordylophora lacustris*), which had attained an extraordinarily luxuriant development. Upon and between these polyps dwelt innumerable microscopic animals such as Vorticella and rotifers, and larger forms, such as naids, entomostraca, and water snails. There was clearly here no lack of food.

Here, however, Crenothrix also abounded; here were growing the large brown flocks, such as were found in the service beyond. Mussel shells and polyp stems were covered with a thick felt of iron bacteria. Nothing was free from Crenothrix, which was present in all stages of its development.

It was further proved, by comparing the materials filtered out by flannel screens, that the water of the river, instead of being improved during its journey to the filters, became, biologically speaking, much worse.

We may turn next to De Vries's account of the conditions beyond the filters, in the basins and conduits of filtered water. This again is rendered almost literally.

Crenothrix in the Rotterdam Reservoirs of Filtered Water.

"Whenever the brown flocks of Crenothrix are found in the service pipes of a water supply, they are a sign of imperfect or faulty filtration. Through good sand filters properly operated, neither the spores of Crenothrix, which are among the largest of those of any bacteria, nor yet the suspended organic particles which furnish by their putrefaction the chief source of nutriment for iron bacteria in water supplies, can pass.

Obviously, no sources of organic contamination ought to exist between the lowest sand layer of the filter and the tap. The use of wood ought therefore to be avoided as far as possible in building reservoirs for filtered waters. The pipe joints, which are often calked with oakum, likewise afford a source of nutriment, and swaying fibres of oakum are very often found covered with Crenothrix. This particular source of contamination appears to have been hitherto overlooked.

As has been stated above, Crenothrix first appeared in Rotterdam in considerable quantities in the spring of 1887. This was the first spring after the thirteen new filters had been added to the system. We have already explained how, in consequence of the excessive demands made for a number of years upon the old establishment, a water strongly contaminated with Crenothrix was supplied to the filters. Still, with good filtration, the flocks of Crenothrix ought to have been held back just as completely as the coarser animal remains. Compared with the river water, the service water ought to have been enriched in dissolved organic matters, but not laden with bodies in suspension. Nevertheless, a sand filtration is never such a perfect process, even under the most favorable circumstances, that the degree of purity of the water to be filtered can be entirely a matter of indifference. Local and temporary disturbances may be reduced to a minimum, but they can never be wholly excluded. In order to make this plain, we must consider somewhat more fully the precise phenomena of filtration.

In 1874, a Commission was appointed to investigate filtration in Rotterdam thoroughly, under the local conditions.[1] This Commission found that the purification of the water occurs by no means equally throughout the entire depth of the sand, but chiefly in the uppermost layer, rich in microscopical organisms, especially diatoms, which layer is gradually formed during filtration. The more completely this layer is developed, the more perfectly will the filter do its work, but at the same time the greater becomes its resistance to the passage of water. This resistance finally becomes so considerable that it is no longer profitable to run the filter. The filter must now be cleaned, i. e. this uppermost layer must be removed, and then the process begins again. This is the universal experience in filtering drinking water through sand. It has been most thoroughly and fundamentally worked out by Piefke in his "Principien der Reinwasser-gewinnung vermittelst Filtration," and "Aphorismen über Wasserversorgung."

[1] Rapport van de Commissie tot onderzoek betreffende het water der Drinkwater-leiding te Rotterdam, Juni, 1875. The microscopical investigation of the sand layers was conducted by Professor P. Harting, in Utrecht.

This author has shown that it is chiefly the jelly-forming bacteria which endow the sand with its filtering capacity. They convert the uppermost layer of sand into a sort of jelly, and develop in the layers next below, much slower, yet in such a way that the latter participate to a certain extent in the filtration, and so that after the cleaning of the filters they can very quickly develop into the peculiar filtering jelly. Without the aid of organisms, sand does not filter. Undoubtedly it will hold back the coarse things which cannot pass through its pores; but bacteria and their germs, as well as finely divided clay, pass freely through its pores so long as these are bounded only by hard and smooth quartz grains. In order to retain such things as these the surfaces of such grains must be sticky, and they become so only through the bacterial jelly referred to. It follows from this, that during the first days after each cleaning the bacterial layer is only imperfectly formed. During this period, therefore, the water must be allowed to trickle through very slowly. Even then it will be almost inevitable that bacteria, and therefore the germs of Crenothrix, shall penetrate into the deeper and non-glutinous layers of the sand, from which they may be swept out later under a more rapid rate of filtration. Every temporary rupture of the bacterial jelly layer during filtration will have the same result. Single germs find passage, and are then no longer held by the sterile under layers. Recognized causes of such gaps are, first, air bubbles rising from below, where they had remained in the sand after cleaning, or after emptying the filter. Again, worms and other animals bore through the membrane, eels break it up, etc. The more undisturbed the jelly layer is, the more perfect is the filtration. The freer the water to be filtered is from burrowing animals, from bacteria and germs, the rarer are these breaks, and correspondingly smaller is the number of organisms which, at each break, can thrive in the deeper layers.

So far as Crenothrix is concerned, it is affected rather in respect to its nutrients than its germs. To keep out the latter is practically impossible; the nutrients, however, need be excluded only so far that they shall not support a perceptible growth. This can be done safely and very completely by the filtration through sand of a well settled river water. In spite of the slight unavoidable disturbances, the water of the Maas can be so purified by sand filtration as to make impossible the multiplication of Crenothrix in the service system. This is established by the prolonged experience of the second Rotterdam supply (Delftshaven), as given above.

Investigation showed that the contaminations of the filtered water

with Crenothrix occurred from two quite different sources. The first was connected with the defects of the new filters and with the new conduit built at the same time. The second lay in the fact that the filtered water was conducted from this conduit, through a part of the old conduit system, to the reservoir of filtered water and the pumps. We must treat these two sources of contamination separately. The first produced only a relatively small contamination. The second, however, furnished in 1887 nearly the whole amount of the flocks detected in the filtered water reservoir, and passing into the service pipes. Let us consider, therefore, in the next place, the new filters and their effluent conduit.

The grounds of the Rotterdam Water Works lie close to the river, and outside a great dike which bounds them on the entire northeast side. Such ground is well known to be much inclined for the first few years to yield under the weight of new structures. Cracks in the walls are the result; nor are they wanting in other structures laid upon the soft earth of Holland. To what extent these crevices might have been avoided by some different kind of filter construction does not here concern us. The fact is, that after a year's operation every one of the thirteen new filters exhibited such crevices. They allowed the unfiltered water to pass from one filter directly to the filtered water basin of a neighboring one whenever the difference in level furnished pressure enough. During the cleaning, such differences of level are obviously almost unavoidable. Similarly, under certain circumstances, they permitted the pressing into this space of water from a neighboring ditch.

The entrance of Crenothrix in such ways was established by direct observation. For this purpose a filter (No. 21) was emptied of water, and the sand and its supports were removed in places, so as to expose the basin for filtered water, and the sides. The water that streamed in here and there through cracks was examined, either directly or by being caught in bottles, and in both cases numerous large and small flocks of Crenothrix were discovered. The microscopical organisms accompanying them, among which were observed actively moving Pandorina and other green algæ in full activity, showed plainly that the inflowing water had come from the neighboring reservoirs of unfiltered water, viz. the ditches, filters, basins, or the upper layers of the sand.

The germs thus introduced had not, however, during one year's working, given rise to any considerable vegetation. Here and there, especially near the cracks, the wall was covered with large slimy

masses of the sulphur bacterium, *Beggiatoa alba*, and similar masses could be found upon the timbers which support the sand. Other organisms were not wanting in these masses, especially the iron bacteria, *Cladothrix dichotoma* and *Crenothrix Kühniana*. They gave to the masses upon the walls, but not to those upon the timbers, an intense yellow coloration. The Crenothrix, however, forms here the usual flocks. From the effluent basins of the separate filters, the filtered water runs in a long conduit, common to all, which lies under the canal of unfiltered water, and conducts the effluent water to the pumps. The wall of this conduit was still quite free from any vegetation, with the exception of one small spot in the neighborhood of a little crack, where Crenothrix occurred. Through this conduit, therefore, probably those contaminations alone were conveyed which had entered as such through cracks in the walls of the filters.

The amount of these contaminations was almost insignificant, as was proved by direct tests made by a flannel screen. At different times such screens were placed in this conduit, sometimes at the effluent opening from single filters, sometimes in such places that the water from several or from all the filters must pass through them. The screens filled completely the entire passage and retained all contaminations. Upon them were invariably found in great quantities filaments of Crenothrix, besides innumerable infusoria, diatoms, worms, débris, etc., but the larger flocks of Crenothrix were a rare phenomenon here in 1887.

As stated above, the chief source of the increased quantity of Crenothrix in the effluent basins was that section of the conduit, lying between the new conduit and the pumps. This section had not been newly built, but was a portion of the old conduit, which, since the beginning of the works in 1874, had served to convey the filtered water from the old filters to the pumps. It was one hundred and twenty meters long, about two meters high, and one meter wide, and contained a row of transverse timbers to strengthen the walls. In spite of these, the walls were in many places broken, and thus permitted the entrance of unfiltered water from the older neighboring filters now disused.

The high degree of the contamination in this conduit was detected as follows. The conduit was completely shut off by flannel screens at its beginning and its end. The former served merely to remove the suspended matter from the water before it flowed into this conduit. If this old conduit had been clean, the second screen must have remained clean. This, however, was not the case. On the contrary, this

screen, after a short time, was thickly covered with the brown flocks of Crenothrix. This portion of the conduit system poured out every day masses of Crenothrix so considerable that the water which flowed from it into the reservoir of filtered water had always to be filtered through metal screens, and these within twenty-four hours often became completely choked up with iron bacteria. From these screens were often collected, in enormous quantities, large flocks more than a centimeter long. These results induced the Commission to visit this conduit. It was easy to inspect the walls in the neighborhood of the old gates, now closed, which formerly admitted the water from the four adjacent filters, since these places were directly accessible. It was only necessary for this purpose to lower the water to a certain depth. To penetrate far from these places was, however, more difficult, since for this the water had to be pumped out as completely as possible, and this could be done only at night. Besides, the conduit was but ill adapted to such a journey on account of the numerous cross timbers within it. They succeeded, nevertheless, in overcoming these difficulties, and in actually observing with their own eyes the principal seat of the vegetation of the Crenothrix.

In the next place, it is to be noted that the living population of the walls, which was almost never absent in the conduits of unfiltered water, was wanting here. There was not a trace of sponges, mollusks, or bryozoa. The rich vegetation of Crenothrix which covered these organisms there, was not therefore to be expected here. On the contrary, the iron bacteria were here growing directly upon the beams and walls. These were everywhere covered with a brown growth, which for the most part was very thin, and only here and there bore the larger flocks of Crenothrix. The walls, however, were covered in streaks with well developed patches of iron bacteria, which spread over them like a felt of velvet, and on microscopical examination proved to be composed entirely of large, dark brown flocks of Crenothrix, sometimes a centimeter high. Out of the brown sheaths, the colorless jointed filaments were protruding in all cases. Upon the beams the vegetation was apparently younger, or at least less well developed; the brown threads were the rarer, the colorless the more frequent. It is possible, however, that they are more easily swept off by the stream here than from the side walls of the conduit. As soon as this source of contamination was recognized as such, the Commission advised that the conduit in question be disconnected, and replaced by a new cast-iron pipe. Accordingly, the authorities ordered this to be done, and the new pipe was ready for use in the spring of the next year. The result agreed with

their expectations, the amount of Crenothrix in the city service diminishing suddenly and very appreciably. This decrease, however, was by no means so great as had been hoped, for the enemy had meanwhile established itself in other parts of the conduit of filtered water. The new conduit, which after one year's use was still free from it, yielded in the two following years gradually increasing quantities of brown flocks. An investigation in August, 1889, showed that there was no accumulation of sponges, mussels, or bryozoa, and that the flocks of Crenothrix were attached directly to the walls, not only in the conduit, but also in the new reservoir of filtered water near the new engine-house. The closeness and thickness of the felt-like growth was different in different places, and some portions had been already cleaned during the summer. At one point there was a smooth dark brown felt a few millimeters in thickness, extending over several square meters; at another, the wall was merely brown, but sprinkled with larger scattered flocks as much as a centimeter in height. Bunches of Crenothrix were continually detached by the stream, and fine metal sieves had to be used to remove these as completely as possible.

Thus there had arisen, within the first three years of operation of the new works, an infection with iron bacteria, which moreover, even after the old conduits had been entirely disconnnected, maintained the original evil, but fortunately not to such an extent that complaints were serious. On the contrary, the Commission believes that the evil will never again reach its original magnitude, although it can be fully overcome only by the improvement of the filters which are concerned.

In conclusion, the biological results of this portion of the investigations may be briefly summarized as follows. The animal life which was found upon the walls of the conduits of unfiltered water was wanting here. Sponges, mussels, polyps, and bryozoa were nowhere observed. With them were absent all those organisms which supported themselves among these; and this, too, not only in the new conduits during the first three years of operation, where they came under observation, but also in the old conduit, after being in use for more than a decade.

On the other hand, Crenothrix had succeeded in establishing itself upon the walls of the reservoir of filtered water. In the first year, the Commission failed to detect it, or detected traces of it only; in the second and third years, it increased to a suspicious extent, and overspread great patches of the submerged portion of the walls with its gently waving vegetation. This was either thin, and dotted here and there with larger scattered flocks, or attained a thickness of several millimeters. In the old conduit these growths were more than a

centimeter in thickness, and continually shed off, into the water flowing by, innumerable large flocks. It would be an important question to discover what velocity of the stream, under given conditions, most favors the growth of Crenothrix. Unfortunately, the Commission could not make any thorough investigation in this direction. It appears, however, as if the very slow movement in the basins of filtered water beneath the filters does not suffice for its luxuriant development, for it was not in these, but in the *conduits*, that it attained its richest development.

The Danger of Contamination of the Water with Crenothrix before Filtration.

How does it happen that Crenothrix was able to develop so luxuriantly in the filtered water of the main system of Rotterdam, while it has not yet established itself in the system at Delftshaven, though the latter draws its supply from the same source?

A good sand filtration frees the water from the suspended organic and inorganic particles; it cannot, however, affect the quantity of dissolved materials. The iron bacteria, however, live upon dissolved matters, whether these are present as such in the water, or, as in masses of bryozoa, etc. penetrated by Crenothrix, arise on the spot from dead, putrefying remains. The cleanest water will be able to support a growth of iron bacteria if only it contains a sufficient quantity of dissolved organic materials. Moreover, this quantity does not need to be at all large. As has been stated already, Winogradsky found that the addition of 0.005 to 0.01 per cent of calcium butyrate or sodium acetate to the well water of Strassburg sufficed for the nourishment of iron bacteria. This is a degree of dilution at which, as is well known, most organisms cannot thrive. And when so limited a quantity of food stuffs suffices in culture experiments in which the water is comparatively seldom renewed, how inconceivably small must be the necessary quantity in a water which, as in the filtered water conduits, moves swiftly and continuously in a quick stream past the bacterial growths, and, accordingly, instantly makes good every loss, however small.

Whenever the water of a water supply is rich enough in dissolved organic matters, the water of the canal and the pipes may become covered with Crenothrix. On account of the marvellous reproductive powers of this organism, only single spores are necessary in order to begin the first growth. Such spores obviously can never be entirely

excluded. Fortunately, the water of the Maas does not contain quantities of dissolved organic matters adequate to support the vegetation of Crenothrix in the filtered water reservoirs. The case of Delftshaven, already repeatedly referred to above, proves this. At the same time, Winogradsky's experiment, cited above, shows that a slight increase will suffice to exceed the dreaded limit. The water must, therefore, become so changed between the river and the filters, that it is adapted for the nourishment of Crenothrix.

Let us now consider from this point of view the vegetation in the basins of unfiltered water, and in the canals which conduct it to the pumps, and finally to the filters. In the settling basins, the water is supposed to stand twenty-four hours in order to secure complete clarification. In consequence, however, of the vastly increased consumption, it has been necessary for several years past to curtail this time appreciably. A large new basin is indeed projected, but meanwhile the settling of the river water in the old basin is incomplete. In so far as the suspended matters are of organic origin and are not deposited, in so far, obviously, will they through their putrefaction increase the content of the water in dissolved organic materials.

In the conduits, the water passes in the dark for a distance of six hundred to seven hundred meters. Almost the entire surface covered by water is, in these passages, furnished with the animals described. Their excreta, their dead bodies, readily undergo decomposition, and furnish a rich source of dissolved organic materials.

It is therefore an inevitable consequence of these conditions, that the water before filtration is charged not only with suspended, but also with dissolved organic substances; and the slower the stream, and the more vigorous the life in the conduits, the greater is this contamination. The current depends upon the consumption of water in the city, the activity of the organisms upon the season and the temperature. In winter, therefore, the slow current, in summer the active metabolism in the masses of organisms, produce an increase in dissolved organic materials. The question now arises whether this increment is such that it may be regarded as having an influence upon the increase of iron bacteria. In order to make possible an opinion on this point De Vries has employed the results of chemical analyses, which since the first years of the service have been made, almost without exception, every week. From these figures he has calculated the averages for the separate months of each year, and then, in order to make the results more easily comparable, has computed the average for the corresponding months of each three successive years. The variations

between single observations thus disappear for the most part, yet by no means completely, and the separate variations afford no sufficient explanation. In a general way, however, he considers that the table gives an unequivocal answer to the question at stake.

TABLE SHOWING THE AMOUNTS OF POTASSIUM PERMANGANATE REQUIRED TO OXIDIZE THE ORGANIC MATTER IN ONE LITRE OF THE ROTTERDAM DRINKING WATER.

Months.	A 1877–1879.	B 1880–1882.	C 1883–1885.	D 1886–1888.	D — A
January	7.5	6.7	6.7	7.4	−0.1
February	6.6	7.1	6.2	6.9	0.3
March	6.4	7.6	5.9	6.9	0.5
April	6.0	5.7	5.1	7.4	1.4
May	5.2	4.7	4.6	5.6	0.4
June	3.6	4.0	4.9	5.9	2.3
July	4.2	4.6	3.5	5.2	1.0
August	4.2	4.4	3.4	5.5	1.3
September	4.2	5.1	3.7	4.7	0.5
October	5.1	6.6	5.1	5.6	0.5
November	7.4	7.2	5.7	7.0	−0.4
December	7.4	7.2	8.0	7.9	0.5

The figures of the table signify milligrams of permanganate ($KMnO_4$). The column D — A shows the difference between the columns D and A, and therefore represents the increase in organic matters in the course of the twelve years under investigation. If we estimate the increase by comparing column D with either of the other three, we shall obtain similar results. Apart from the variations referred to, it is evident at a glance that in the early years of the establishment the quantity of organic matter in summer was about half as great as in winter. The latter quantity has altered but little during these years; on the whole, it has increased a little. In summer, on the other hand, the increase has been very significant. This increase in quantity of organic matters has not been accompanied by a corresponding increase in inorganic matters. De Vries calculated the weekly estimations of the latter in

the same way as before, and finds an average increase of about 10% over the original quantity (185 milligrams per litre in winter). This increase is least in the summer months (about 8%), and greatest in the winter (as much as 15%). It does not keep pace, therefore, with the increase in organic matters.

Any complete comparison of the figures in the table with those of Winogradsky's experiment is impossible. Roughly speaking, they seem to have nearly the same value. From earlier analyses of the Rotterdam Water Works, De Vries estimated that on the average one milligram of permanganate equalled about five milligrams of loss on ignition. According to this, the figures in the table would indicate about 0.002–0.004% of organic matter (ash free). In the experiment cited (Winogradsky's) the nutrient solution of the iron bacteria contained from 0.0025 to 0.008%.

The above table extended to January 1, 1889. In the spring of that year the cleaning of the settling basins and canals began for the first time.

De Vries concludes, therefore, that it is a fact that the dissolved organic matter in the water has increased during the operation of the filters. He does not assume by any means that this increase has occurred in the settling basins and the conduits of unfiltered water exclusively. Obviously, the wooden beams which support the sand, and the vegetation of Crenothrix itself, even in the conduits of filtered water, furnish an amount not to be neglected. Yet the iron bacteria are satisfied with such an extremely small amount of these foods that every source of organic matter must be regarded as injurious, or at least as dangerous. It is difficult for the biologist to execute the sentence of death upon this wonderful population, which is able to thrive in the dark chambers of our water works. Nevertheless, De Vries regards the execution of this sentence as inevitable, if we would exterminate the Crenothrix from water supplies where it has once become established.

Crenothrix in the Water Supply of Lille.

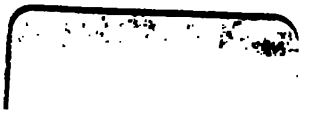
In the spring of 1882 there suddenly appeared in the water supply of Lille, in France, a pollution so extensive that the water for a time could not be used. For some years previous the water had occasioned anxiety on account of its reddish color, bad taste, and disagreeable odor, but it had not produced serious trouble until April, 1882. At that time it became so bad that in certain quarters of the city it could

not be used, and for some time afterward every heavy rain was followed by a fresh disturbance. The water bore a rusty scum, easily strained out. Ferruginous deposits also formed in the reservoirs and some of the distributing mains. The water became so bad that even the tram-horses at one time refused to drink of it. This attack of Crenothrix was studied by A. Giard,[1] who found little to add to the work of Zopf except that the microgonidia appeared to him to be motile, and furnished at one time with a flagellum. These afterward pass into an irregular *Merismopædia* stage, and still later into zoöglœa. From the zoöglœa the usual filaments are finally produced. The source of the drinking water of Lille appears to have been a series of wells in a marshy region, and the conditions appeared to Giard similar to those that prevailed in the original Tegel supply of Berlin. He therefore urged sand filtration as a remedy, and advised towns about to institute water supplies to avoid ground waters as a source of supply.

Crenothrix in a Water Supply of Rome.

The latest case which has been reported is in one of the water supplies in the vicinity of Rome.[2] The "Cometo Tarquino" system became so infested with Crenothrix that the water was ill-smelling and no longer potable. The difficulty appeared to arise, not in the springs or reservoirs used as the source of supply, but in a conduit forming a part of the main leading to the city.

[1] A. Giard, Comptes Rendus, 1882, p. 247.

[2] Bentivegna e Sclavo, Un Caso di inquinamento in una Conduttura di Acqua potabile per lo Sviluppo della "Crenothrix Kühniana." Roma, Diregione Sanità Pubblica, 1890. — Giornale della Reale Società Ital. d' Igiene, Milano, 1890.

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